PROCEEDINGS

OF

ENGINEERS' SOCIETY OF WESTERN PENNSYLVANIA.

PITTSBURG, PA.

VOL. II.

SEPT., 1882, 1883, 1884.

CONTENTS.

VOL. II.

May.	Joint Session of the American Society of Mechanical Engineers with the Engineers' Society of Western Pennsylvania, at rooms of Engineers' Society of Western Pennsylvania.	
	Address of Welcome by President Wm. Miller, of Engineers' Society of Western Pennsylvania.	
	Reply of President Sweet of the American Society of Mechanical Engineers.	
"	Report of Committee on Natural Gas.....	T. P. Roberts, Ch'rm'n. F. C. Phillips. W. S. Tarboe. A. E. Hunt, Secretary.
"	Discussion of the Report on Natural Gas.	
Sept.	Will a Drowned Person be Raised by the Discharge of a Cannon?	Jose de Cueto.
Oct.	A Specimen of Cast Iron Pipe.............	James H. Harlow.
Nov.	Continuation of the Discussion of the Report of the Committee on Natural Gas.	
	Natural Gas...............................	Wm. Metcalf.
Dec.	Critical Methods of Detecting Errors in Plane Surfaces	J. A. Brashear.

PETROLEUM.

By Max Livingston.

A paper read before the Engineers' Society of Western Pennsylvania, October 17, 1882.

Three events conspire to stamp the year 1859 as a landmark in history, one a military and political feat; the other two intellectual achievements. But long after the battle of Solferino, which sealed the doom of Austrian supremacy in Northern Italy, will have been forgotten by all save the antiquarian and tactician, the first publication of "The Origin of Species," and the inauguration of the Commercial Petroleum Era, will serve to commemorate that year to all friends of progress.

The axiom, that great discoveries and inventions do not originate suddenly, but are the outgrowth of more or less persistent labor and research, is exemplified in this instance, showing the analogy between scientific and commercial attainments.

The theory, which the poetic genius of Goethe foreshadowed, and the logical mind of Lamarck formulated and enunciated, was finally proven and established by Darwin after having pondered it for nearly a quarter of a century.

The discovery of petroleum can by no means be claimed as an achievement of modern times. The holy fires of Baku, worshipped for ages not only by the people living near the shores of the Caspian sea, but even by the more distant Persians, have been sustained by apparently inexhaustible petroleum stores. From Pliny and other Roman writers we learn that petroleum was known and utilized to some extent in their days, under the name of Sicilian oil, coming from Agrigentum, the present Girgenti. More remote antiquity will have us believe, that the bricklayers of Babel must have considered it a valuable ingredient of mortar, and Herodotus, the so-called father of history, informs us that the Egyptians prized it for embalming purposes, and supplied their wants from the wells of the Island of Zante, which, rumor says, have lately become the property of a Bostonian, thus seemingly reversing the supposed order of things of prehistoric times.

The existence of petroleum on our continent is said to have been spoken of within ten years after the landing of the "Mayflower." Curious enough, the claim of priority has to be conceded to one of the youngest producing districts, namely: the promising region of Allegheny county, N. Y., which, only fifteen months ago, was a terra incognita to the oil prospector. The first authentic report of the existence of petroleum in our own state was generally attributed to the Commandant of Fort Duquesne, who, in a letter to General Montcalm, described the ceremonies of the Seneca Indians, in which said oil played an important part. From "Stowell's Petroleum Reporter," we learn that nearly 30 years prior to that time, Charlevoix, in his Journal of May, 1721, quoting Captain de Joncaire as authority, mentions a "fountain at the head of a branch of the Ohio (the Allegheny), the water of which is like oil, has a taste of iron, and serves to appease all manner of pain."

The production and use of petroleum, however, not only preceded the white explorers and first settlers, but is supposed to have preceded the Indians, for modern research, pointing to the remains of old wells and pits along Oil Creek, in which oak trees, whose roots have been preserved by the oil, have grown and decayed, impels us to assume that petroleum must have been known and collected in considerable quantities by a more civilized race than the Indians—if the story about the oak trees cannot be explained by the building of the pits around the trees, in which case the hypothesis of a migration from Asia to America would be strengthened.

Returning to more recent times we find that the old settlers collected the oil either by means of blankets spread over the streams or pools to absorb the oily substance floating on the surface, or by a series of rudely constructed troughs. Primitive as this mode of production was, it yet formed no mean source of revenue to some operators on account of the high price commanded by the article as a desirable liniment. As such it was almost exclusively used under the name of Seneca, Rock, or British oil or Naphtha, until about 1850.

The precursor of our petroleum has been coal oil, made by destructive distillation of bituminous coal or shale, the manufacture of which was then successfully carried on in Scotland. Its introduction and diffusion here

naturally directed the attention to our oil springs, and caused our late townsman, Mr. Samuel Kier, to make the first attempt to convert natural crude oil into purified illuminating oil. For this purpose he procured a cast iron still of one barrel capacity, and obtained the oil from the salt well of Lewis Peterson, near Tarentum.

With our present knowledge of the dangerous character of certain burning oils, we find it surprising that these trials, in which no separation was made of the light and extremely dangerous hydrocarbons, should have been attended by such encouraging results as to stimulate Mr. Kier to enlarge his works with a second still, having an increased capacity of three barrels.

Permit me to relate an incident illustrating the magnitude (?) of the oil trade at that period. A certain Isaac Huff, lessee of a salt well situated opposite Tarentum, now owned by Kipp & Lockhart, of Pittsburgh, was greatly annoyed by constant accumulations of oil in his well. To turn his source of trouble into one of profit, he concluded to collect the "nasty stuff" and take it to Pittsburgh, which he did as soon as he had accumulated three barrels. Arriving with this, his first shipment, in Pittsburgh, he applied to Mr. Charles Lockhart for advice about selling the oil, who named Kier as a probable, and some wholesale druggists as possible, purchasers. After a few hours Huff returned with depressed spirits. Kier, he said, had refused to make even an offer, and the druggists were not willing to buy so large a quantity at one time. Huff's disappointment at Kier's refusal was not greater than Lockhart's surprise, who was sufficiently acquainted with the business to know of the growing demand for Kier's oil and the limited supply. This fact, in conjunction with the knowledge of the vast proportions the coal oil trade was assuming, likely induced Lockhart not only to buy Huff's present stock, but also to make a contract with him for all the oil he could produce during the continuance of his lease, at the price of 31½ cents per gallon, delivered in Pittsburgh. It can safely be assumed that this was the first petroleum contract for "future delivery," and without entering too closely into details I will simply remark that it yielded the purchaser a handsome profit. But not only that; a few years later he bought a controlling interest in said well, which kept up its production, and the experience and knowledge acquired then made him become afterwards one of the pioneers of the petroleum business.

This digression, by way of example, will indicate the narrow limits of the oil trade of only thirty years ago, which did not permit or encourage the pursuit of refining of petroleum beyond the tentative state until a greater difficulty, the question of supply, was disposed of. For having solved this problem the thanks of mankind are due to the late Mr. E. L. Drake, of New Haven, Conn. It was he who, in June, 1859, after experiencing the full share of disappointment and vexation meted out by fate to every explorer, began sinking the first artesian oil well, for which remarkable venture he selected a farm near Titusville possessing one of the most prolific oil springs in the neighborhood. But the mere fact of making a beginning did not end his trials. He was harassed and beset by difficulties which put to a severe test his mental, and well nigh exhausted his and his friends' financial resources, before his patience and perseverance were crowned with success, and called into existence one of the most important industries of the age.

This memorable event occurred on the 29th of August, 1859, when his drill, dropping into a crevice of oil at a depth of 70 feet, inaugurated the production of petroleum of commerce.

The transformation scene following closely upon the heels of Drake's success, along the quiet hillsides and valleys of Venango county, baffles description.

It was a motley crowd that then thronged those regions, and it may be said with as much propriety of the pursuit of wealth as of politics, that it makes strange bedfellows.

Merchants and adventurers, industrious workmen and professional tramps, from all parts of the country, rushed to the new Eldorado in search of fortunes; and wherever any "surface indications" promised success, the busy hands of the mechanic were soon at work erecting derricks and boring holes. At the end of 1860 over 200 wells were in successful operation, making the production of that year about 500,000 barrels, all of which was brought to the surface by pumping.

But crazed and intoxicated as our fortune-hunters had become by the sudden disclosure of so much wealth, there yet remained a surprise in store for them, namely, the discovery of the flowing well, the first one of which was struck in February, 1861, yielding 400 barrels per day.

When, however, this was followed in rapid succession by several others, gushing forth over 3000 barrel per day each, amazement knew no bounds, and excitement was carried to fever heat. The days of John Law seemed to have returned with all their grotesque oddities and picturesque grandeur, with all their agreeable surprises and cruel changes of fortunes. Men grew rich over night, only to find themselves poor again shortly afterwards, and of all the fortunes made at that time, but very few have been retained.

The marvelous growth of the petroleum business has become one of the wonders of our century, and an attempt on my part to outline the development of the oil territory in the United States alone, would far exceed the limits of my time. In passing I will

simply state that the production, which in 1859 may have reached 2000 barrels, attained the enormous amount of 27,358,000 barrels in 1881, which, in turn, will be overtopped considerably by 1882, and this refers to the oil regions of Pennsylvania and its offspring, Alleghany county, N. Y., only.

For the convenience of those, who take a deeper interest in my subject, I have compiled from "Stowells Petroleum Reporter,," and Dr. P. Schweitzer's lecture, to both of which I am indebted for valuable information, tables of statistics, which will be appended to my paper.

Under all circumstances, no matter whether the production fell short of the demand, or exceeded it to such an extent that the oil had to run to waste by thousands of barrels daily, it remained an important question, where productive wells could be located.

In the earliest stages of the business the prospector was guided or misled, as the case might have been, by surface indications, and the most ridiculous—nay superstitious—notions. Every successful operator had his pet theory, which, with a great deal of self-complacency, was expounded to a credulous constituency. The so-called practical man, with his contempt for "theorists," became thoroughly theoretical, after a fashion, and mapped out in every direction, and in all possible curves and angles, the supposed course of the subterranean channel or oil belt. According to Mr. Henry E. Wrigley, in his report upon the Oil Regions of Pennsylvania, "the most valuable of all these propositions was that made by Mr. C. D. Angell, of Franklin, Pa., who, observing in 1871, hata number of the oil producing spots when noted upon a map would be intersected by a straight line, whose bearing was about north 16° east, proceeded to define this line carefully upon the ground, and while he discovered at intervals upon it some new producing spots, yet failed to establish the theory advanced of continuous oil belts. As subsequent investigation has proved, the truth of his suggestion lay in the fact, that the general course of the grand current which deposited the sand rock was in the direction named; the error of his statement, in the fact that nature never works with absolutely straight lines, and that the beds of sand rock are deposited at intervals only, as may be seen to a greater or less extent in the bottom of any running stream."

Geologists—the protests of some oil producers notwithstanding—assure us, that oil does not exist in a series of underground lakes or rivers, but is lodged in porous sandstones or sands. Assuming this to be a fact, these rocks can only have been deposited by river, deep sea or shore currents, but to which, whether to one or all of these currents the oil group owes its existence, is a matter of doubt yet. Some of our foremost geologists, among them Mr. J. F. Carll, in his valuable report on the "Geology of the Oil Regions," incline to the opinion that the oil group is a shore deposit; others maintain that the conditions under which sands have been deposited have varied in different localities. Mr. Charles A. Ashburner, for instance, in his excellent work "The Geology of Mc Kean County," and in several lectures asserts, that the Venango sands were undoubtedly shore or shallow water deposits, laid down by rapid and shifting currents, whereas those of the Bradford region were "possibly deposited in deeper water by a slower and more constant current."

We are also indebted to Mr. Ashburner for an admirable, succinct comparison of the structure of these oil sands, which I quote, thinking it may impart welcome information on a befogged subject. Mr. Ashburner says: "The Bradford sand consists of a gray and a white sand of about the same coarseness as the ordinary beach sand of the Jersey coast; compact, yet loosely cemented. The average thickness of the sand is about 45 feet, and from top to bottom the sandy strata change but little in their general character. It is only when specimens from the successive layers are placed side by side and closely examined, that any difference in structure can be recognized. The grains of sand are regular, vary but slightly in size, color and the quantity of cementing material which holds them together in their rock bed. The same homogeneousness, which characterizes the vertical section, is found to exist over a considerable horizontal area. In fact but little change is found to exist in the sand obtained from wells 15 miles apart, or in the sand from intermediate wells * * * The characteristics of the Venango sands are quite different. * * * * A productive Venango sand consists of a white, gray or yellow pebble rock; the pebbles being loosely cemented together and generally bedded in fine sand. The rock is open and porous. The interstices between the pebbles and the sand grains are extensive and capable of containing a large bulk of oil; but this character does not maintain itself over any extended area. Areas of such sand are small and scattered and are separated by sand beds, possessing a character belonging to the unproductive sands. The Venango sands are not homogeneous over any considerable area and are frequently very heterogeneous in section. The thickness of the sand varies; in one locality the upper part of the sand may be pebbly and of productive character and the lower part fine and contain no oil, while but a short distance away the conditions may be reversed. The difference in the structure of the sands, when considered in connection with their relative productiveness, is a strong argument in support of the view which has been accepted by the best informed of our

geologists that the sands are only reservoirs or sponges, which serve to hold the oil, coming almost entirely from an inferior formation to which it is indigenous."

And this brings me to a question which has caused much more unfruitful speculation and controversy than the sands and the manner of their formation, namely: the origin of petroleum.

To explain this, in the highest degree interesting and important question, theories without number have been propounded, but although men eminent in scientific circles have wrestled with the problem, we are far from a satisfactory solution. For this reason it would be more gratifying to myself to dismiss the subject as too embryonic, were I not conscious of the curiosity which many of the gentlemen present feel, to hear something, no matter how hypothetical, regarding the formation of petroleum.

I shall but briefly refer to a few plausible theories. According to one, the oil is indigenous to the sand-rock and is supposed to have been elaborated by nature from organic matter, which during the palaeozoic ages, when submarine plants and premordial animals flourished in abundance, were deposited simultaneously with and in the sands. But when and by what means these hydrates and mollusks could have been converted into oil so completely that not even a trace of them has been found, and how this oil, during incalculable ages subjected to revolutions of the most violent character, could have been preserved, is beyond our comprehension.

Diametrically opposed to this hypothesis is the theory that oil is a product of condensed gas, distilled, so to speak, at great depth, where the temperature is sufficiently high, from organic deposits in the beds of the silurian and devonian formation. The gas thus generated is forced to the upper and cooler strata, where it is absorbed, and as far as possible condensed into liquid, in the sponge-like reservoirs, the sandstones. Analogous to this, only combatting the organic origin, is the theory propounded by Prof. Mendelejeff. Calling into requisition the nebular hypothesis of Kant and Laplace, and assuming that the interior of the earth contains large masses of metal and carburetic compounds, he draws the following conclusion: "Through some of the fissures in the crust of the earth, occasioned by the upheaval and depression of the surface, water percolated to the carburetted metals and acted upon them at high temperature and elevated pressure, thus forming metallic oxides and saturated hydrocarbons. The latter rose in the form of vapor" and became converted into liquid as before described.

Still another theory claims an intimate relation between coal and petroleum, contending that the latter is a product of the former, from which it has been expelled more by pressure than heat, and considers our anthracite coal fields, for instance, a residuum of petroleum.

But all these theories have many vulnerable points, which to expose this evening would lead us too far. Neither can I spare the time, nor have I the inclination, to speculate on the probable amount of oil likely to be produced yet, until the explored oil territory has been drained. Experts have given us figures and opinions, supported by ingenious arguments, which in many instances were refuted before the printers' ink had time to dry. The oil territory, distributed all over the globe, is so vast that we need not fear its early exhaustion, and I venture to assert that its classical domain, so to speak, will for some time to come be the State of Pennsylvania.

Leaving the speculative field, and passing over the question of transportation which, before the present gigantic system of pipelines intersected our oil regions with a network, and, spider-like, stretched its iron arms for hundreds of miles in all directions, was as interesting as serious, we at length come to the refining of petroleum.

In its crude state petroleum is a complicated liquid of a great number of differently compounded hydrocarbons, many of which are in a high degree volatile and inflammable. It is composed of about 85 per cent of carbon, and 15 per cent of hydrogen. Its condition as to color, specific gravity and quality, is by no means uniform in the diferent districts of its occurrence.

Some places produce an oil of light specific gravity, clear in color, like transparent amber, and almost odorless, in fact so pure that it is frequently burned in lamps in its original state; other regions bring forth an oil of a heavy specific gravity, a dark, opake, greenish color verging on black, and possessing an offensive odor. Again, we find an oil of heavy specific gravity, which is by no means offensive to the eyes and nostrils. Oils of this nature—like all heavy oils—are not valuable for refining purposes. They are, however, all the more prized by the manufacturer of lubricating oils.

The Pennsylvania petroleum (I have no reference to the small amount of heavy oil produced in the neighborhood of Franklin) has a specific gravity of from 41° to 48°, according to the producing localities. The Bradford, or upper region, produces the heavier oil, while the so-called "lower country" (Butler, Parker and Clarion) furnishes a superior article of from 46° to 48° gravity.

The conversion of crude into an illuminating oil—safe, odorless, and giving a brilliant light—is the function of the refiner. It is not my purpose to sketch the development of the refining business from its infancy—the advent, as we have seen, of the diminutive still of one-barrel capacity, which likely caused its

The Pennsylvania petroleum (I have no reference to the small amount of heavy oil produced in the neighborhood of Franklin) has a specific gravity of from 41° to 48°, according to the producing localities. The Bradford or upper region produces the heavier oil, while the so-called "lower country" (Butler, Parker and Clarion) furnishes a superior article of from 46° to 48° gravity.

The conversion of crude into an illuminating oil—safe, odorless, and giving a brilliant light—is the function of the refiner. It is not my purpose to sketch the development of the refining business from its infancy—the advent, as we have seen, of the diminutive still of one-barrel capacity, which likely caused its manipulators more uneasiness than the colossal retorts of 2000 or 3000 barrels give us today; nor can I outline the numerous varieties of apparatus, with their trifling or important modifications, now in existence. No matter how much the different appliances vary in detail, the refining of petroleum finally resolves itself into the following principal operations:

1. Fractional distillation (evaporation and condensation).
2. Application of chemicals, and washing with water.
3. Settling or clearing, and perhaps expelling dangerous, inflammable gases.

With but few exceptions the distillation of petroleum is effected in large iron stills by application of direct heat. As a rule, an oil still and its principal appurtenances consist of the following parts:

A large horizontal or vertical cylinder, made of iron or steel plates from $\frac{1}{4}''$ to $\frac{3}{8}''$ thick, resting on and partly surrounded by brick-work, similar to our ordinary cylinder boilers.

The furnace or furnaces, which are also constructed like those of the boilers referred to.

A large outlet, or vapor-escape pipe, the prolongation of which is

The long condensing pipe, or worm. For the purpose of having this immersed in water, it is generally placed in

A vessel, commonly called the condensing tank, which has to be constantly supplied with cold water to cool and condense the vapors traveling through the condensing coil. At the end of this coil the product is inspected at intervals as to gravity and color, "cut off," or separated, and accordingly led into different receiving tanks.

Before charging the still, it is desirable to have the crude oil as free from water and other impurities as possible.

Shortly after the still is fired up, evaporation of those exceedingly ethereal fluids known as cimogene, rhigolene and gasolene, begins, which are allowed to escape in the gaseous form, as it is impracticable to condense them in an apparatus as here described.

Gradually, as the rising temperature increases evaporation, vapors are generated which readily condense into liquid, and the more readily as they become heavier with the progression of the process.

The early stages of the operation, during which the distillate flows clear and colorless, require comparatively little attention, but when the oil grows dark and heavy, pains should be taken to further that process known as cracking or decomposing, which causes the heavy vapors to separate or split up into heavier and lighter ones, the former falling back into the still, the latter passing off through the vapor pipes. Thus the distillation is generally continued until the bulk in the still has been reduced to about 6 per cent, which residue has usually a specific gravity of 18°, and is called tar or residuum.

The different products of such a distillation are classified as follows:

Light Benzine or Naphtha, density or gravity	80 70°
Heavy " " "	70-64°
Distillate for Refined Illuminating Oil, density or gravity	64-41°
Tar, or Residuum (which, logically speaking, is no product), density or gravity	18°

This residuum is drawn off through a pipe near the bottom of the still.

The dividing lines are drawn rather arbitrarily, and I may remark that inasmuch as it is difficult, chemically speaking, to define the line between steel and iron, it is impossible to locate it between oil and benzine. While by proper manipulation we can extract a good and safe oil from benzine, it is, on the other hand, possible to transform crude oil almost entirely into benzine and tar.

Benzine, which serves an endless number of purposes, is subjected to various treatments, according to the wants to be satisfied. Giving it only a passing notice, I can state that benzine is the friend and foe of gas. In the character of a friend it carburets gas of poor illuminating power; as a foe its principal product, gasoline, enters the market as a strong competitor of gas, displacing it often from the streets in the most populous districts.

The distillate, when issuing from the condensing pipes, is, as a rule, impregnated with tarry matter and inflammable gases, imparting to it a greenish color and an offensive odor, to expel which it is transferred into a large vertical cylinder with conic bottom, called a agitator, where, by means of an air blast, the oil is thoroughly agitated or shaken up, so that every particle of it is brought in contact with sulphuric acid. The objectionable impurities are thereby to a great extent destroyed, or precipitated with the refuse acid as soon as the agitating ceases. After this waste material—which, by the way, is valu able to the acid restoring factories, or for fe

tilizing purposes—has been drawn off, the oil in the agitator is subjected to a thorough wash with water, which is followed by an agitation with a solution of caustic soda or some other alkali, for the purpose of neutralizing any trace of acid which the water may not have affected. The drawing off of the waste alkali water generally finishes the process in the agitator, whence the oil is transferred into open tanks to settle and brighten, and, if necessary, to have by various devices its fire-test improved, preparatory to entering upon its mission.

By fire-test is understood the temperature of the oil at which it gives off dangerous, inflammable gases. Naturally, the higher the temperature the more readily will oil volatilize. The point at which, by our method of testing, oil emits the first vapors which, if brought in contact with a flame, burn like a flash, without setting the oil on fire, is called the flashing point, or flash test; whereas, it is called the fire-test when the quantity of vapor thus thrown off is sufficient to communicate the flame to the oil. The difference between the flashing and burning points, by the Tagliabue, or open instrument, is generally about 22 degrees. The reason for the great variation in the test of oil finds its explanation in the fact that the constituents of petroleum, nearly all of which belong to the paraffine series, have a widely diverging boiling point—that of the lighter members being exceedingly low; that of the heavier ones correspondingly high. But while the fire-test increases with the gravity, the quality, for the purpose under consideration, only does so to a certain point, after which it degenerates; for, whereas oil of low fire-test and light specific gravity burns exceedingly brilliant, that having a high fire-test and heavy gravity, as a rule, gives a dull light, chars the wick and smokes. It therefore follows that a combination of light gravity with high fire-test—the former to insure brilliancy of light, the latter safety—should be the desideratum of the consumer.

Many of you have doubtless experienced that the ordinary refined oil does not possess these requisites. The standard of safety, which in our state is only 110° fire-test, is not often transgressed, but that this alone does not satisfy any more a great many consumers is proven by the large and rapidly growing demand for a better article than one which will simply pass official inspection, and as a consequence we now find numerous brands of so-called fancy oils appealing to public favor, some of which are in a high degree deserving of patronage.

The public, however, has to understand that for safety and brilliancy of light it should not rely exclusively on the refiner and inspector. It must heed the fact, that that enemy of civilization, uncleanliness, is one of the most prolific sources of accidents. A clean burner is an indispensible requisite; without it the best of oil will not give satisfaction. If the air or vent holes in a burner are clogged by particles of charred wick or dust, the flame is not sufficiently supplied with oxygen, and must therefore burn dull. At the same time the burner, which otherwise would be kept cool by the free circulation of air, is subjected to a great heat, which by various means can lead to accidents.

To what extent carelessness and stupidity are responsible for serious calamities it is superfluous for me to state. The newspapers all over the country furnish sorrowful reports daily, and will continue to do so as long as the oil-can is called into requisition to facilitate the kindling of fires.

An important branch of the petroleum business is the manufacture of lubricating and paraffine oils and paraffine wax from tar or residuum, which has been brought to great perfection within the last five years. The tar, when distilled, yields a large amount of oil varying greatly in color and density, the product being light and thin at the beginning, and growing gradually darker and heavier or thicker as the distillation progresses. In proportion to its gain in specific gravity the oil grows richer in wax, which is extracted by means of reduction of temperature and pressure. Besides thus obtaining a valuable article, in great demand for the manufacture of candles, matches, chewing gum, &c., the extraction of the wax improves the quality of the oil for lubricating purposes.

The economical use of petroleum as fuel is a question which agitates the minds of many experts. The present time with its enormous production and exceedingly low prices is certainly a propitious one for experimenting; but calculations as to the comparative cost between petroleum and coal should not be based on present prices, because in the event petroleum became a successful competitor with coal, the demand would before long largely exceed the production, and unless this should increase in the same ratio, the price of oil had to advance considerably.

A brief reference to the influence of petroleum on civilization may not be out of place.

As a suggestive instance picture to yourself the long, dreary winter evenings in the farm houses all over the country, when the tallow candle and pine knot reigned supreme, and, comparing with them the cheerfully lighted homes of to-day, with their intellectual recreations, which could not be indulged in formerly without doing violence to the eyesight, you will agree that petroleum has added greatly to the welfare of a very large and important class of our citizens. If it has been such a boon here, how vastly more must it have improved the condition of those millions upon millions of people all over the globe rescued, so to speak, from darkness by the introduction of petroleum. Its dissemination in

those semi-barbarous districts must, without doubt, prove itself one of the most potent agents of progress.

And how far-reaching has been the influence of the introduction of petroleum on trade, commerce, and the industrial arts? I can merely touch upon this matter and shall not attempt to recount at length the wonderful changes which have taken place, nor recite the phenomenal improvements in machinery, tools, &c., directly attributable to petroleum, for which the perfection of the art of drilling artesian wells, and the complete revolution in the manufacture of barrels, cans, boxes, &c., furnish notable instances.

But not enough with these matter of fact achievements, the refining of petroleum has become the means of proving most conclusively that doctrine of political economy which avers that the work requisite to supply the wants of humanity will eventually be performed by concentration of capital and combination on a large scale of machinery and labor through the agency of powerful corporations, either for their own benefit or that of society. Where else do we find this doctrine as strikingly illustrated as in the

Exports.

YEAR.	Crude Oil.	Refined Oil.	Naphtha.	Lub'g. Oil.	Tar, &c.	Total value
	Gallons.	Gallons.	Gallons.	Gallons.	Gallons.	Dollars.
1862	5,828,129					
1863	155,874					
1864	9,908,654	12,791,518	438,197	....	9,746	10,807,560
1865	12,293,897	12,704,825	480,847	....	15,928	16,586,631
1866	16,057,943	32,255,921	673,477	....	26,921	24,895,899
1867	9,344,248	62,686,657	224,576	....	50,312	24,442,441
1868	10,029,659	67,909,961	1,517,268	....	38,685	21,840,071
1869	12,425,366	84,403,492	2,673,094	....	....	31,071,216
1870	9,955,066	98,350,753	5,422,604	....	245	32,666,731
1871	9,859,038	132,608,955	7,209,592	....	5,345	36,899,568
1872	13,559,768	122,539,975	8,092,635	541,419	438,186	34,058,390
1873	18,439,407	158,102,414	9,743,593	748,699	781,074	42,050,756
1874	17,776,419	217,220,504	9,737,457	1,244,305	1,827,798	41,245,815
1875	14,718,114	191,551,933	11,758,940	1,173,473	2,752,848	30,078,568
1876	20,520,397	204,814,673	14,780,236	963,442	2,581,404	32,915,786
1877	26,819,202	262 441,844	15,1[illegible]0,183	1,601,065	3,196,620	61,789,438
1878	26,936,727	289,214,541	16,416,621	2,304,624	4,216,527	46,730,972
1879	28,601,650	368,597,467	19,524,582	3,186,561	4,827,522	37,255,467
1880	36,748,116	286,130,557	15,115,131	5,618,009	3,177,636	34,505,643
1881	40,430,108	444,655,615	20,655,116	5,053,862	3,756,018	48,556,103

Production, Price, Producing Wells and Stocks of Pennsylvania Petroleum.

Years.	Yearly production in barrels of 42 gallons.	Average price at wells, or of Pipe Line Certificates.	Number of producing wells at end of year.	Stocks in the Oil Regions at the end of year.
1859	2,000	$20 00	203	....
1860	500,000	9 60	406	....
1861	2,113,600	49	609	....
1862	3,056,600	1 05	812	....
1863	2,611,300	3 15	1,015	....
1864	2,116,100	9 87½	1,218	....
1865	2,497,700	6 59	1,421	....
1866	3,597,700	3 74	1,624	....
1867	3,347,300	2 41	1,827	....
1868	3,646,100	3 62½	2,030	....
1869	4,215,000	5 63¾	2,233	337,658
1870	5,260,700	3 89	2,436	554,626
1871	5,205,300	4 34	2,639	532,000
1872	5,939,000	3 64	2,812	1,084,423
1873	9,891,000	1 83	3,045	1,625,157
1874	10,950,700	1 17	3,240	3,705,639
1875	8,787,500	1 35	3,174	3,550,200
1876	8,968,900	2 56¼	6,000	2,824,739
1877	13,135,600	2 42	8,458	3,127,837
1878	15,165,400	1 19	10,337	4,615,299
1879	19,741,600	85⅞	11,960	8,470,490
1880	26,032,400	94½	14,700	18,928,430
1881	27,358,000	85⅞	18,300	26,019,704
1882				

Standard Oil Co., where else the teachings of the most advanced political economists, that in the ratio as such concentration of capital takes place, implying as it does the expropriation of the many smaller capitalists by a few greater ones, the application of technological science, and the systematic development of the resources of the earth progress, more impressively exemplified? Unconsciously, perhaps, the able men at the head of this gigantic corporation have rendered an important service to social science, a service destined to be more appreciated as civilization advances, and which the future historian will fully acknowledge when recording the economic development of this period of transition.

As it is proverbially only a step from the sublime to the ridiculous, I may be permitted to conclude by relating a short, frivolous dialogue, which appeared, appropriately illustrated, in one of the pictorial papers of New York nearly 20 years ago. The subject represented a young miss of aristocratic descent, and a young gentleman. The latter, with unfeigned haughtiness, makes the following inquiry: "Is it really true Miss Golddust, that your sister is engaged to be married to Mr. Oilrich of Venango county?" "I guess not," was the naive reply, "Pa says whale oil and kerosene don't mix." Well,

gentlemen! events have proven that the proud Pa of this damsel was right. Figuratively speaking whale oil and petroleum did not mix. Look at New Bedford! What has become of its once powerful and numerous fleets of whalers? Swept from the ocean almost simultaneously with the general introduction of petroleum, only a few crafts are left to-day to tell of their former glory. This sudden collapse of the whale-fisheries, and the breaking up of many other trades from the same cause, form the dark side of the picture, and impress us with nature's inexorable law of the survival of the fittest in the struggle for existence.

ON ACTUAL STRENGTH OF RAILROAD BRIDGES.

BY F. MELBER, C. E.

A paper read before the Engineers' Society of Western Pennsylvania, Nov. 20, 1882,

GENTLEMEN: The subject of our considerations this evening and the substance in brief I offer to discuss is the conduct of our railroad bridges, if under the influence of their full load. In how far I succeeded in the choice of the title for this paper, of this I leave the members of the Society to judge. In speaking of strength of railroad bridges, or any other structures, we understand the ratio in which the capacity of the material of the bridge or structure to resist is to the straining or strains caused by the loads of whatever kind the latter may be. Large numbers of tests of various kinds and to serve various aims have been made during the last 25 years in different countries to investigate the conduct of many kinds of materials, particularly of iron and steel, in resisting tensile, compressive and shearing strains, or a combination of them, and the results obtained thereby, as for instance those of Wöhler and Bauschinger, justify the opinion that the factors of safety, as used up to the time those experiments were made, in dimensioning the railroad bridges and all structures of a similar character were not those which could promise the safety required. The well known fact that we may break a stick of wood or a piece of metal in repeatedly applying a certain part of the force which would break them at once has been completely verified by the experiments alluded to. Our railroad bridges are undergoing the same process whenever the moving load passes over, and the adaptation to those experiments seems obvious. On the other hand, only a few years ago Manderla, in Forster's *Bauzeitung*, showed a systematical calculation of secondary strains in rivet-connected bridges, caused by deflection of the fully-loaded structure; the figures so obtained in one instance amount for top chord in middle to about 14 per cent., bottom chord in middle 16 per cent. and for web members in middle 5 per cent. of the ordinary strains, if the center line of the members meet in one point. It has been shown that in cases of excentricity of members, which may be frequently observed in older structures, above figures will rise as high as 50 per cent. of said strains, the influence of the dead weight of the members, however, has not been regarded, as far as I could find out,

and, as we shall see hereafter, may with right be neglected without danger of making a mistake of any account. In consideration of all those investigations mentioned, we may ask ourselves if, or in how far, they may be adaptable for our pin-connected railroad bridges. As to the strength of materials, we, of course, answer in the affirmative. Until about 25 years ago it was considered as a fact that a bar which stands a certain strain one time satisfactorily will sustain the same any number of times again. Accordingly the bar was tested, and in straining it gradually, the strain per square inch causing its rupture was taken as its ultimate strength against tension, compression, etc. Although it was, as it is acknowledged by every engineer, that sudden thrusts or pulls produce an injurious effect, in 1858 A. Wöhler, now director of railroads in Alsace-Lorraine, drew the attention of the engineers to the necessity of making experiments on the strength of materials against often repeated but not suddenly applied straining, in Erbkam's *Zeitschrift fur Bauwesen*, 1858. Provided with all means to be wished in making exact and extensive experiments, Wöhler, by commission of the Government, made experiments of the before-mentioned character during a space of 12 years, when Spangenberg was chosen his successor. The materials tested were steel and iron. The result of those experiments was what is called Wöhler's law, saying that the rupture of a strained bar may be caused by often-repeated appliance of a smaller force than that defining its ultimate strength of the meaning mentioned before. From this law we infer at once that the straining of the materials may be allowed to increase the more the less the number of repetitions we have in view. Soon after the publication of Wöhler's law, Fairbairn in 1864 made experiments with a rivetted girder, which I cite to illustrate the law in figures. Fairbairn first loaded that girder to quarter of the ultimate strength of the metal and it did stand 1,000,000 repetitions satisfactorily, after 313,000 more repetitions with one-third of the stress causing immediate rupture, the girder broke down. Many other examples of a similar nature may be found in the reports from the laboratory for tests in Munich. As it is more my intention to investigate the manner in which the members of a truss will resist the strains caused by the weights or loads on the bridge than to find out the ultimate strength of the materials used, the above remarks concerning the latter subject, although of equal importance, as the former may be sufficient, and I invite your attention in considering the changes in the manner of straining of the members of a structure, which may and will take place as a consequence of the elasticity of the materials of which the structure consists, and of a certain resistance on the point of connection, of which we will have to speak again at the proper place. For the sake of a more general understanding of the subject, I think it proper to make a few remarks on deflection in general, and the regard which is and has to be paid to it practically in cambering the trusses of railroad bridges. Let g in Figure 1 be the weight of a rolling load, as, for instance, the weight on a truck wheel of a railroad car, which has to be conveyed from one abutment at m to another at n over the beam $m\ n$, let P be a force just large enough to move it slowly. Then, because the beam, which is supposed to be perfectly level, will deflect or bend down as soon as the load begins to move on it, the force P, in consequence of this, will have to be increased if otherwise the motion of the weight toward

the opposite abutment shall not cease, since not horizontal motion alone will lead to the right, but also a certain distance a in a vertical direction must be passed through. Now, if it is required for some reason to carry out said motion with considerable velocity, we discover at once two extra sources of strains the beam will have to resist besides those due to its dead and the rolling loads. One of those sources mentioned furnishes a vertical reaction from the part of the beam, forcing the weight vertically up, which action will strain the beam in proportion to the angle of deflection, it will therefore increase the nearer the weight moves to the abutment n. The other of the sources produces a strain depending only on the velocity of the weight, it is the reaction of the beam, due to the centrifugal force obliging the weight to move in a course prescribed by the deflected form of the beam. Let a be the amount of deflection as shown in Figure 1, g the acceleration due to gravity and v the velocity of the weight G per second in a vertical direction, then the reaction first alluded to may be found from equation No. 1 in the appendix to this paper, and if r represents the middle radius of deflection and v this time the velocity of the weight G in the direction of the beam, then the other reaction of the beam spoken of will be found from equation No. 2 in the appendix. Since, as was mentioned before, the velocity of the weight in a vertical direction is uniformly accelerated, therefore the nearer the weight comes to the right abutment, as we are assured it will, the more will the increase of the reactions of the beam against the weight in upward direction amount to, whilst, as we have heard, the reactions caused by the centrifugal force are constant. Figure 2 illustrates the manner in which both gravity and centrifugal forces will strain the beam, the ordinates of the oblique-hatching representing strains due to counteraction against gravity, whilst the lower ordinates of the vertical-hatching refer to reactions against centrifugal force. Although if we would apply what we have found to take place for our beam to cases of practice as to uncambered bridges; for instance, we might see that in ordinary cases that extra straining for 15′ panels may not exceed a produced panel-load of about 1,800 ℔s. per 30 miles velocity, we nevertheless learn from Figure 2 that in extraordinary cases the members near the end or, the nearer the end of the truss the more, may receive very considerable overstraining and consequently will wear out faster than they would do if the truss would be straight under the influence of its total loading. But these are not the only reasons why we should provide as we do against the deflection of our railroad bridges. The main reasons are of a practical nature. As the trusses uncambered form a concave line (m n Figure 2) when loaded, the materials forming the track of the road will wear out unequal and faster at the ends m n and on the abutments near m and n, than on other places of the road; further, the vehicles or axles, wheels, etc., of the latter are unequal and overstrained and wear out faster than they would do if conveyed from m to n in a horizontal line. In adding all these we may say that for theoretical, as well as for practical, reasons we are doing well, if we give our trusses a convex form, slightly cambered, so that the track if parallel to the chords, take the Pratt truss for instance, will be horizontal. This we will attain if we try to get the versed sines of the cambered form of the truss as near as possible equal to the amount of deflection of the truss under total load. If we assume that unavoidable small inaccuracies

in fixing the lengths of the truss members take place in the same manner for each member if measurements are carried out by the same hand for all the truss members then those inaccuracies are of no influence on the camber or on the form of the truss, because all dimensions will have either slightly increased or decreased as the case may be. Several rules or methods, to assume the versed sines of the cambered shape, are in use, which, however, differ from each other, the most general way is to take a certain fraction of the length of the span, without paying any regard to the depth of the truss, the consequence of this is that the larger the depth of a truss the more considerable the difference or length between the top and bottom chords has to be made, and as we shall see hereafter a truss being cambered this way don't needs become straight or horizontal, unless by mere chance in one case—namely, if the elasticity of the material taken, together with the ratio of depth to the length of the truss, would give the same versed sine as the rule, before-mentioned and nearly generally used, will give. Our practical feelings tell us that in reference to the deflection of a beam or truss an increase of depth will cause a decrease of deflection and reversed. As in the course of the discussion of this question about cambering it may add to a general and clear understanding in the matter, I by turns will refer to a diagram of a 150-foot span which I assumed and which may be seen in its cambered and deflected form in Figure 3 drawn exaggerated. The dotted lines of course represent the imaginary form of the truss, neither dead nor live loads are acting. I will briefly mention that the strains, which will be found in the table added to the appendix, correspond with a loading of the bridge consisting of 2 74-ton freight engines, followed by 2,000 lbs. per foot freight, besides 1,400 lbs. dead load. We next will have to answer the question what are the uses of the deflection of a truss? There are two causes with which we ave to deal sure in every case. The one, although for small or short

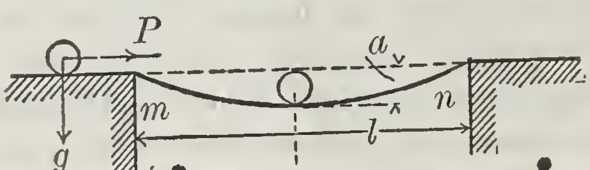


Fig. 1

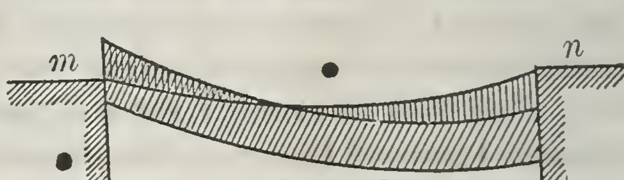


Fig. 2

bridges of very little influence is the deformation at the connection point of the members of the truss. The transmission of the strains from one member to another takes place by means of bearing on the connecting pin. The play, which for practical reasons has to be allowed, would make it necessary for the bearing to take place along one touching line, but the elasticity of the metal establishes a touching or bearing surface which causes a set, that although very small, will count up with the number of connecting pins in the truss and may therefore be neglected as mentioned for small trusses of 100 feet or under. The other of the causes alluded to is the elongation or shortening of the members under tension or compression respectively, depending likewise on the properties of the material—that is, on its elasticity. If we are able to give a formula or rule including those two causes of deflection, we certainly possess good means to fix the proper amoun‘ of camber in any given case. As to the deformation at the con-

necting pins we may safely say that a setting may take place as long as there is not bearing surface enough to allow a pressure of about 18,000 lbs. per square inch. The formula in Appendix No. 3 gives then the amount of set asked for and is based on the latter supposition. The well known formulæ (Appendix No. 4) gives the amount of elongation or shortening of the tension—or compressed members respectively. As will be observed at once, the latter formulæ (No. 3 and No. 4 appendix) represent constant figures as soon as strains per square inch and the length of the pieces in question are constant, and if the length should vary, a recalculation is not required. The Web members add consequently direct to the deflection a certain known amount per apex, whilst the chord members contribute indirect. The top chord shortens and the bottom chord elongates to a known amount, both forming parts of concentric circles, the versed sine of the bottom chord circle is the contribution of the chords to the deflection. If we add both direct and indirect contributions together as we have received them by the described operations, we shall have obtained the proper amount of deflection or camber. For the 150 feet span for which in the appendix this method of fixing the amount of camber is followed a deflection of 0.1434 feet was found, representing $\frac{1}{1060}$ of the length of the span, the depth of the span is assumed to be 25 feet, and according to formulæ (No. 5 and No. 6 in appendix) a cambering of said truss to the amount of $\frac{1}{1200}$ of its length, as generally assumed, would require a depth of the truss of a little more than 32 feet. Having found the causes and the amount of deflection on the trusses of a railroad bridge, we will proceed to investigate the retroactive effect of that deflection on the members forming the trusses. As the bearing surfaces of the truss members on the pins are very small in comparing them with the strain to be transmitted thereby, a deformation will take place as we have already heard, although within the limits of the elasticity of the materials. For total loading of the bridge this deformation causes a set of $\frac{1}{128}$ of an inch per two pins of four inches diameter as shown in the appendix. Dead load alone will cause a deformation of $\frac{1}{4}$ to $\frac{3}{4}$ of that amount, as the case may be. From these facts it is evident that a motion of the member under part of its strain round the pin in question could not take place without damaging or destroying the touching surfaces of pin and bar or post. From Figure 3 we see that if a truss is obliged to deflect—that is, to go from the cambered in Figure 3 dotted form over to the horizontal position shown in full lines, that in doing so the truss members are forced to change their relative positions towards each other, which cannot be carried out, however, without either a turning of the members round the pins, or if this should not be possible, by bending of said members in a certain manner to be spoken of hereafter. Since deflection takes place as often as the truss is loaded—that is, with each train that passes over the bridge, the operations alluded to will repeat themselves very often, and in case of a turning of the truss members the wearing out of the pin surface at those places consequently ought to be perceptible. The latter, however, is not the case. I examined for this purpose the pins of a railroad bridge which had been used over 12 years, but I found in feeling the

surface of the pins as smooth as if never used, neither could I see anything uneven. On the other hand, by watching pins of bridges still in use no turning could be observed. It may not be improper to call back in our memory an every day's experience for to illustrate, to some extent at least, the conduct of a truss member if obliged to move to a small amount round its connecting pin. I mean the opening and shutting of a door, the latter having a loose hinge connection, we noticed, no doubt very frequent, that if we add to the friction at the hinge the hinge frame will bend if we move the door, and the turning motion at the hinge will follow as soon as the bending strength of the hinge frame exceeds the resisting friction of the hinge movement. In consideration therefore of the small amount of motion of the truss members by deflecting of the bridge, and considering further the large amount of friction spoken of and the considerable length of the members offering not very much resistance in bending to overcome said friction we may safely say that our pin connected bridges as far as deflection and strains arising therefrom are concerned will behave and act similar as stiff connected railroad bridges. We will now proceed to investigate of what kind those secondary strains are, and how they act in brief. As we have learned that in consequence of the elasticity of the materials every one of the Δs constituting the form or shape of the trusses will change its sides, and consequently its angles, which cannot be done without obliging the ends of all members at one apex to make a joint motion, the pin included, round the center of the latter to an amount equal to the angle of deflection, as we may call an angle which the tangent

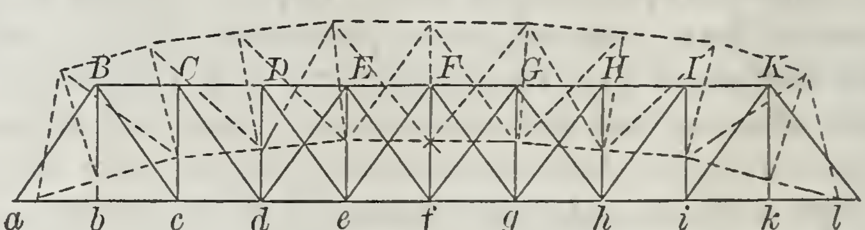


Fig. 3

on the elastic line at the end of the deformed member, forms with the original axis—that is, with the straight line connecting the pin centers after deflection of the truss. To illustrate this, Figure 4 shows exaggerated those angles for the left half of our 150-foot span, and the manner in which the different members will bend from the end toward the middle of the truss, the other half of course will be symmetrical. If those angles of deflection are found the amount and kind of extra straining for each member may easily be determined. We therefore will see next how those angles are to be found in restricting ourselves to the description of the course of the problem, giving the corresponding mathematical development in the appendix. The basis of our operations is to find a relation between the occurring changes of the sides of the Δs forming the truss and the ways of the apexes of the Δs due to those changes. In Figure 4, for instance, we consider the side *Bc* as fixed and at *a* and *c*, vertical to *aB* and *cB*, we attach strains the sum of moments of which in reference to *B* will be zero. Those strains alluded to may be of any amount whatever if they are only in reversed proportion to their lever arms. The application of the law that for all forces which are in equilibrium the sum of their work done = 0 or the work done by one of said forces in opposite directions equals

the work done of the rest of them gives us the change of one of the $\angle s$ of the $\triangle$ in question (compare Equation No. 7 in appendix) this change may be received positive or negative according to a deflection of the member to the right or the left, as may be seen from Figure No. 4. These changes, however, are not to be taken for the angles of deflection, but there exist such relations between these both kinds of $\angle s$, that if we know one of the angles of deflection for one of three apexes in the truss, forming the

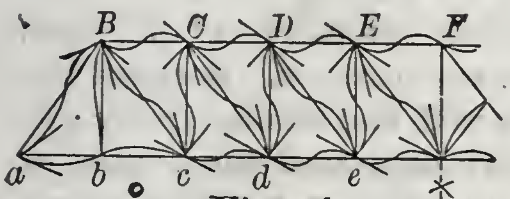


Fig. 4

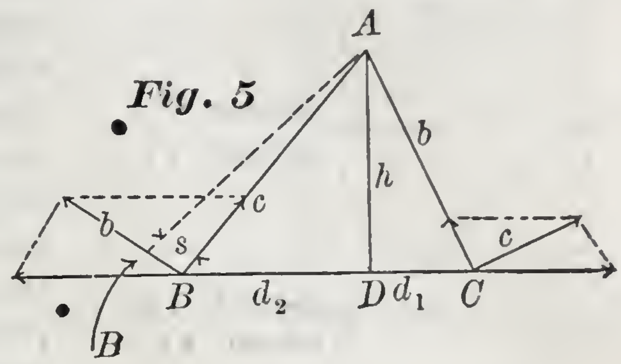


Fig. 5

corners of a $\triangle$ of the latter that we may find those of all other members joining at said apexes by means of said changes of $\angle s$ (see equation No. 8 in appendix). We therefore assume at each apex one of the angles of deflection and introduce it in the equations of the moments at each end and for each member joining at said apexes, and since the sum of all moments round each apex has to be zero, we finally receive (from equation No. 8 in appendix) all the angles of deflection wanted. Those moments referred to are due to the oblique fastening of the members at each end and in opposite directions, and to the influence of its dead weight as an equally distributed load. As in all such cases the form of the elastic line, depending on the section of the member and on the quality of the material of which it consists and on the moment of the acting forces, will have to be found first. From this we get a relation between said moments, vertical transverse strains and angles of deflection of the member (as per Equations No. 9 and No. 10 in appendix). The moment in regard to any intermediate point of the member (as given in Equation No. 11 in appendix) will lead to the relation between the angles of deflection at each end and the bending mo ments of the same places as Equations No. 12 and No. 13 will show. These a moments are those alluded to before, which will give in connection with Equation No. 8 the angles of deflection, and with the latter the final moments and fibre strains produced by the deflection of the trusses, which, together with the axial strains received by the customary calculation, will give the total strain in the member (as per Equations No. 14 and No. 15), and the comparison of this so received total strain with the capacity of the material of the bridge to resist these straining in the sense as shortly alluded to in the introduction, will furnish a proper judgment as to the strength and durability of our railroad bridges. As the moment in the middle caused by the dead weight of a member since the latter is fixed at each end will be found very small, causing for 30′ panels of top chords a fiber strain of about 400 lbs. per square inch in case 15″ built [s are used and for inclined Web members as in the Warren system for instance, still less, and since this bending moment will yet be reduced near the middle of the member in question, because of the different direction

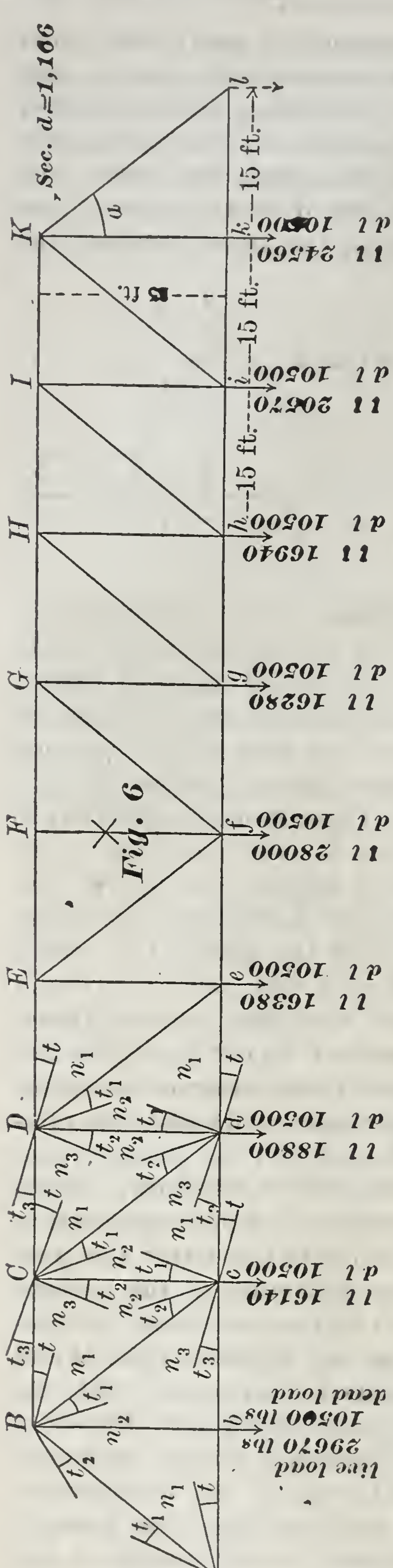


Fig. 6

of the unequal angle of deflection at the ends, we may for the members under compression on account of the surplus of section toward the ends, where the larger moments are, neglect the influence of deflection, if we only make those members deep enough so that no excess of strain at their ends may take place, what we always are at liberty to do. I think it advisable that the leaning end post more than any other member of a truss should be taken care of. Because the bolster being stiff and strong enough to resist any turning round the center of the bolster pin without bending, being pressed down on the rollers by ½ of the weight of the bridge and its loads: the latter force can choose any leverage it pleases to keep the bolster from changing its horizontal position in following the elongation of the bottom chord, whereby the rollers next the bridge side will receive the larger part of the reaction on the abutments, and the end post consequently may be considerably strained and its dead weight will thereby act favorably, yet may not be sufficient to counteract the upward deflection of the post. In rivet-connected bridges, where the tension members are or should be comparatively thin and wide, the section lost by the rivet holes is not to be regarded as the cause of a waste because making the bar wider it helps to reduce the fiber strain, caused by bending under deflection of the bridge, and to reduce in the same time the camber of the bridge, since the stretch of a member is in reversed proportion to its section, this is the reason why rivet-connected bridges show really less deflection than those with pin connections, and this no doubt adds considerably to the strength and durability of the rivet-connected bridge. Without paying, however, particular attention to the manner of connecting, we may in sum of what we have heard say that any increase of depth in the section of the truss members of any bridge will add not only to the strength of the member itself, but also to the durability of the whole bridge, and that the efforts

made in this line, as, for instance, to counteract the dead weight of bridge members are unnecessary.

APPENDIX.

Eq. No. 1: $P a = \frac{v^2}{2g} \cdot G$ wherefrom $P = \frac{v^2}{2\,a\,g} \cdot G$.

Eq. No. 2: $P = \frac{G.v^2}{g.r}$.

Let r be the radius of the pin and R that of the pin-hole, farther s the chord common to both circles for a projected bearing length due to 18000 lbs. per square inch, and x may represent the distance of point of bar or pin from its former position, when the bar was not strained, then this distance will be:

Eq. No. 3: $x = r - \frac{1}{2}\sqrt{4\,r^2 - s^2} - \{ R - \frac{1}{2}\sqrt{4\,R^2 - s^2} \}$.

Where s may be obtained from the equation $s = \frac{S}{d.18000}$ if S represents the strain in post or bar and d the thickness of bar or bearing part of post on the pin surface.

Let λ be the elongation of a bar, or shortening of post respectively, and l the length of either one of them, and F be its section, the member being strained by a force P, then if E represents the modulus of elasticity of the material we have:

Eq. No. 4: $\lambda = \frac{P.l}{E.F}$.

Assuming a 150-feet span with strains and sections due to an arrangement of loads and weights on apexes as may be seen from table below, we receive with a modulus of elasticity of 26000000 for lower chord and web members according to Equation No. 3 $s = 2{,}78''$ and $x = 0{,}004''$ which gives an elongation of 0,008" per bar or post on pin bearing, this may be assumed for all bridges where 4-inch pins and same unit strains are used.

Equation No. 4 gives an elongation for each lower chord panel of $\lambda = \frac{10000.15.12}{26000000} = 0{,}0692''$; for main diagonals in trusses of $\lambda = \frac{8900.29{,}15.12}{26000000} = 0{,}120''$ and a shortening for vertical and end post of $\lambda = \frac{6100.25.12}{26000000} = 0{,}0704''$ and for top chord panels of $\lambda = \frac{7600.15.12}{26000000} = 0{,}052''$. In adding all these single influences of the members we find an elongation of bottom chord, due to deformation of pins and eyes of $10.0{,}008 = 0{,}08''$ and due to elongation of the body of the bars $= 10.0{,}0792 = 0{,}692''$ giving a total elongation of bottom chord of $0{,}772''$, and the top chord shortens for $10.0{,}052 = 0{,}52''$ including the projected end posts, while the web members contribute direct from deformation in connections $3.0{,}008 + \frac{5.0{,}008}{1{,}166} = 0{,}058''$ and due to stretch or shortening in the body respectively $= \frac{4.0{,}120}{1{,}166} + 4.0{,}0704 = 0{,}693''$ and the amount of camber will be received in adding to these two last obtained contributions of web members, that is to $0{,}693 + 0{,}058 = 0{,}751''$; the amount resulting from total elongation of bottom chord and total shortening of top chord, the latter, as will be observed, depends mainly on the depth of the trusses. Let now h represent any depth of our 150-feet span, and r be the radius of the circle formed by the bottom chord due to stretch

and shortening of the chords, then it will be clear without the use of a figure, that we receive the following:

Eq. No. 5: $r:(r-h) = \left(150 + \frac{0,772}{12}\right) : \left(150 - \frac{0,52}{12}\right)$ from which we derive the relation $r = 1394,6.h$ and the versed sine of the circle formed by the bottom chord will be consequently:

Eq. No. 6: $sin.\ vers. = r - \sqrt{r^2 - \left(\frac{150,0643}{2}\right)^2} = r - \sqrt{r^2 - 5629,823534}$

and the sin. vers. of total deflection or camber of the truss will be finally $= r - \sqrt{r^2 - 5629,823534} + \frac{0,751}{12}$ feet. From Eq. No. 6 we discover at once, since the increase of depth of the truss causes an increase of r and reversed (according to Eq. No. 5) that the versed sine or camber of the bridge will be in reversed proportion to the depth of the truss, that is it will increase if the depth decreases, and decrease if the depth of the truss increases. In taking 25 feet for h we find $r = 34865,0$ feet and the camber $= 0,0808 + 0,0626 = 0,1434$ feet, or $= \frac{1}{1060}$ of the length of the truss, whilst $\frac{1}{1200}$ of the length (reversing the course pursued above) would require a depth of a little over 33 feet.

Let M be the bending moment of an oblique fastened beam (and as such are our truss members to be considered according to the results obtained from equation No. 7 and 13 below) in reference to any cross-section in a distance of x feet from the left end of the member, farther let Q be the vertical transverse strain in reference to that section, and q may represent the dead weight of the member per lineal foot; farther if M_1 & M_2 are the bending moments in reference to the left or right end of the member and if Q_1 and Q_2 represent the vertical transverse strains for same places respectively than we have the well known relations:

Eq. No. 11: $$\begin{cases} M = M_1 + Q_1\,x + \frac{qx^2}{2} \text{ or } = M_2 - Q_2\,(l-x) - \frac{q}{2}\,(l^2 - x^2) \\ Q = Q_1 + q\,x \text{ or } = Q_2 - q\,(l-x) \end{cases}$$

and M_1 M_2 Q_1 and Q_2 will be obtained from the well-known differential equation giving the elastic line: $\frac{d^2\,y}{d\,x^2} = \frac{M}{E\,\theta}$, if x represents the abscisses and y the ordinates of a system of co-ordinates with its origin at the left end, M represents the above moment of Equation No. 11, and E is the modulus of elasticity, whilst θ represents the moment of inertia of the section. If the angle of deflection to the left is t_1 and at the right end t_2 and if we substitute for M its value from Equation No. 11 then the integration of the differential equation of the elastic line will lead to two equations of the unknown quantities M_1 and Q_1 because for $x = o$ we have $\frac{d\,y}{d\,x} = t_1$ and for $x = l$ we find $\frac{d\,y}{d\,x} = t_2$ and in each case is $y = o$ we receive finally the

Eq. No. 9: $$\begin{cases} Q_1 = + \frac{6\,E\,\theta}{l^2}\,(t_1 + t_2) - \frac{ql}{2} \\ M_1 = - \frac{2\,E\,\theta}{l}\,(2t_1 + t_2) + \frac{q\,l^2}{12} \end{cases}$$ and similar.

Eq. No. 10: $$\begin{cases} Q_2 = + \frac{6\,E\,\theta}{l^2}\,(t_1 + t_2)\,\frac{q\,l}{2} \\ M_2 = + \frac{2\,E\,\theta}{l}\,(2t_2 + t_1)\,\frac{q\,l^2}{12} \end{cases}$$ for the leaning end post

which, besides being obliquely fastened at each end, moves at one end for a certain obtainable distance s, we receive in the same way:

$$\text{Eq. No. 9}a: \begin{cases} Q_1 = + \dfrac{6\,E\,\theta}{l^3}\,[\,l\,(t_1 + t_2) - 2\,s] - \dfrac{q\,l}{2} \\ M_1 = - \dfrac{2\,E\,\theta}{l^2}\,[\,l\,(2t_1 + t_2) - 3\,s] + \dfrac{q\,l^2}{12} \end{cases} \text{ and}$$

$$\text{Eq. No. 10}a: \begin{cases} Q_2 = + \dfrac{6\,E\,\theta}{l^3}\,[\,l\,(t_1 + t_2) - 2\,s\,] + \dfrac{q\,l}{2} \\ M_2 = + \dfrac{2\,E\,\theta}{l^2}\,[\,l\,(2t_2 + t_1) - 3\,s\,] + \dfrac{q\,l^2}{12} \end{cases}$$

To find the changes of the angles in the triangles constituting the form of a truss if the members are under strain, the following has been used as leading principle. Let u represent the change of the angle $B\,A\,C$ in the $\triangle\,A\,B\,C$, the side $A\,B$ having moved in the position of $A\,B$. Now if c vertical on $A\,C$ in C, is a force of as many units as the side $A\,B = c$ of the $\triangle$ and in same way if b vertical on $A\,B$ in B is a force of as many units as the side $b = A\,C$ of the $\triangle\,A\,B\,C$, then we have equilibrium in reference to point A because $b.c = c.b$. Suppose $A\,C$ is immovable, and the sides of the triangle shorten or elongate respectively, as may be seen from Fig. 5, under the influence of the forces in action, the side a to the amount of l_a, and b to the amount l_b, same way c to the amount of l_c, whilst the acting forces are $+\dfrac{bc}{h}$; $-\dfrac{c\,d_1}{h}$ and $-\dfrac{bd_2}{h}$ respectively, and if s represents the way of the force b due to the change n of the angle $C\,A\,B$, that is $s = c.\sin. n = c.n$ (because n is always very small) then we have

$$b.c.n = \frac{1}{h}\,(b\,c.l_a - c\,d_1.l_b - bd_2.l_c) \text{ and}$$

$$n = \left(\frac{l_a}{a}\cdot a - \frac{d_1\,l_b}{b} - \frac{d_2}{c}\cdot l_c\right)\frac{1}{h} \text{ and since}$$

$$\frac{l_a}{a} = \frac{S_a}{F_a E}\,;\ \frac{l_b}{b} = \frac{S_b}{F_b\,.\,E}\,;\ \frac{l_c}{c} = \frac{S_c}{F_c\,.\,E} \text{ if } S_a\ S_b\ S_c$$

are the strains of the members and $F_a\ F_b\ F_c$ their respective sections and E the modulus of elasticity; we finally receive n for this special case from

$$\text{Eq. No. 7:}\quad n = \left(\frac{S_a}{F_a}\cdot\frac{a}{h} - \frac{S_b}{F_b}\cdot\frac{d_1}{h} - \frac{S_c}{F_c}\cdot\frac{d_2}{h}\right)\cdot\frac{1}{E}$$

If $t\ t_1\ t_2\ t_3$ represent the angles of deflection for the members joining at one apex, as shown in Fig. No. 6, and if $n_1\,n_2\,n_3$ respectively are the changes of angles of the $\triangle$ for the corners joining at said apex, then we have the relation as will be seen from Fig. No. 6 (at the end below).

$$\text{Eq. No. 8:}\quad \begin{cases} t - t_1 = n_1 \text{ or } t_1 = t - n_1 \text{ and same way} \\ t_2 = t_1 - n_2 = t - n_1 - n_2 \\ t_3 = t_2 - n_3 = t - n_1 - n_2 - n_3 \end{cases}$$

therefore, if we assume one of the angles of deflection t for one apex, we may, by means of the changes n express all other angles of deflection in the one, which is assumed as unknown quantity. For our 150 feet span for instance (Fig. 6) we may at each of the 10 apexes $a\ b\ c\ d\ e\ f\ B\ C\ D$ and E, assume one angle of deflection t; for to find all the angles of deflection for the truss we only need 10 equations, representing relations between those assumed 10 unknown quantities t. Such relations we receive at once, if we remember that the bending moments as given in Equations Nos. 9 and 10, the so-called "normal moments of continuous girders," contain on the

right side only known quantities except the unknown angles of deflection, and if we therefore for each of the members *aB*, *BC*, *CD*, *DE*, *EF*, *Ef*, *Ee*, *De*, *Dd*, *Cd*, *Cc*, *Bc*, *ac*, *cd*, *de*, *ef*, apply Equations Nos. 9 and 10, and if we further consider that the sum of all moments for members joining at one apex, for their ends at said apex and in reference to the latter, has to be zero, then we are in possession of as many relations between the angles of deflection "t" as apexes were considered, that is in our case 10. In consideration therefore of what has been said here before and in reference to Fig. 6, we have the following:

EQUATION NO. 12.

For $a\,c$ (M_1 at a) : $M_1 = -\frac{2\,E\,\theta}{l}(2t + t_3) + \frac{q\,l_2}{12}$ and $M_2 = +\frac{2\,E\,\theta}{l}(2t_3 + t) + \frac{q\,l^2}{12}$

For $c\,d$ (M_1 at c) : " " " " " " " "

For $d\,e$ (M_1 at d) : " " " " " " " "

For $e\,f$ (M_1 at e) : " " " " " " " "

M_1 for $f\,g$ which is needed also equals M_2 of $e\,f$ as a matter of course

For aB (M_1 at a) : $M_1 = -\frac{2\,E\,\theta}{l_2}[l\,(2t_1 + t_2) - 3s] + \frac{q\,l_2}{12}$ and $M_2 = +\frac{2\,E\,\theta}{l^2}[l\,(2t_2 + t_1) - 3s] + \frac{q\,l^2}{12}$

For BC (M_1 at B) : $M_1 = -\frac{2\,E\,\theta}{l}(2t + t_3) + \frac{q\,l^2}{12}$ and $M_2 = +\frac{2\,E\,\theta}{l}(2t_2 + t) + \frac{q\,l^2}{12}$.

For CD (M_1 at C) : " " " " " " " "

For DE (M_1 at D) : " " " " " " " "

For EF (M_1 at E) : $M_1 = -\frac{2\,E\,\theta}{l}\,.\,2t$; since angles of deflection at $F = o$ we shall not pay any attention to apex F.

For $B\,c$ (M_1 at B) : $M_1 = -\frac{2\,E\,\theta}{l}(2t_1 + t_2) + \frac{q\,l^2}{12}$ and $M_2 = +\frac{2\,E\,\theta}{l}(2t_2 + t_1) + \frac{q\,l^2}{12}$

For Cd (M_1 at C) : " " " " " " " "

For De (M_1 at D) : " " " " " " " "

For Ef (M_1 at E) : " " " " " " " "

The moment for Gf at f equals the moment M_2 for $E\,f$ we therefor omit $G\,f$.

For Cc (M_1 at C) : $M_1 = -\frac{2\,E\,\theta}{l}(2t_2 + t_1) + \frac{q\,l^2}{12}$ and $M_2 = +\frac{2\,E\,\theta}{l}(2t_1 + t_2) + \frac{q\,l^2}{12}$

For Dd (M_1 at D) : " " " " " " " "

For Ee $M(_1$ at E) ; " " " " " " " "

From these equations No. 12 we finally receive the 10 equation between our unknown angles of deflection t. We however think it sufficient to show the example for one apex C for instance, in placing the sum of moments at said apex $= o$ we receive

$$\text{Eq. No. 13:}\quad \left\{\begin{array}{l} \underbrace{+\frac{2\,E\,\theta}{l}\left(2\,t_3 + t\right) + \frac{ql^2}{12}}_{BC} - \underbrace{\frac{2\,E\,\theta}{l}\left(2\,t + t_3\right) + \frac{ql^2}{12}}_{CD} \\ \underbrace{-\frac{2\,E\,\theta}{l}\left(2\,t_2 + t_1\right) + \frac{ql^2}{12}}_{Cc} - \underbrace{\frac{2\,E\,\theta}{l}\left(2\,t_1 + t_2\right) + \frac{ql^2}{12}}_{CD} = o \end{array}\right.$$

Similar relations will be obtained for the rest of the apexes and of the quantities $\frac{2\,E\,\theta}{e}$ and $\frac{ql^2}{12}$ are replaced by their respective known amount in figures, and if all angles of deflection are expressed in "t" of a certain apex, the 10 equation so obtained will be very simple and the angles of deflection easily obtained.

Having found the angles of deflection we receive from Equations No. 9 and 10, Q_1 M_1, and Q_2 M_2, and therefore from Equation No. 11 the bending moment in reference to any cross section in any distance x from the left end of the member. We find the maximum moment or the section where the latter occurs by integration of the differential equation

$$\frac{d^2y}{dx^2} = \frac{M}{E\theta} = \left(M_1 + Q_1\,x + q\,\frac{x^2}{2}\right)\frac{1}{E\theta}$$

we receive

$$\frac{dy}{dx}\,E\theta = t_1 + M_1\,x + \tfrac{1}{2}\,Q_1\,x^2 + \frac{qx^3}{6}$$

and since M will reach its maximum for an x which satisfies the equation $\frac{dy}{dx} = o$, we receive x from the equation of $\frac{qx^3}{6} + \frac{1}{2}\,Q\,x^2 + M_1\,x + t = o$ and we may receive three places where the elastic line changes its course and we will have to try which of these values of x gives the absolute maximum. Since the compressed members of the truss have a comparatively large section compared with the tension members, and consequently will have an elastic line with very small ordinates, the influence of the chord or post stress on the moments just received will therefore not amount to anything considerable, and we shall find the total stress for compressed members per square inch of extreme fiber from

Eq. No. 14: $S = s + \frac{M.}{\theta} y$ where y represents the distance of the extreme fiber from the neutral axis of the section, and s the strains per square inch due to axial compression, and M and θ as named before. Having found the bending moment M for tension members, which generally have a very small section compared with the length, we may reduce it before fixing the fiber strain caused by it, for the axial tension stress of the member may find an ordinate of the elastic line at the place of M large enough to constitute a moment in opposite direction to M as found above. The form of the elastic line is altered by the influence of the axial stress. Our new elastic line is now represented by the equation:

$$\frac{d^2y}{dx^2} = \frac{M_1}{E\theta}\left(1 - \frac{P}{EF}\right) = \frac{M{-}P.y}{E\theta}\left(1 - \frac{P}{EF}\right)$$

where, as will be observed, M_1 represents our bending moment M minus the moment Py caused by the axial tensile stress P, with a leverage equal to the ordinate y of the elastic line; E and θ are as before named. Let

$$k^2 = \frac{P}{E\theta}\left(1 - \frac{P}{EF}\right) \text{ and } f(x) = \frac{M}{E\theta}\left(1 - \frac{P}{EF}\right)$$

where k is constant and $f(x)$ a known function of x, then we will have the equation of the elastic line $\frac{d^2y}{dx^2} - k^2y = f(x)$ and we receive by integration

$$y = A.e^{kx} + B.e^{-kx} + \frac{1}{2k}.e^{kx}\int_o^x f(x)\, e^{-kx}\, dx - \frac{1}{2k} e^{-kx}\int_o^x f(x).e^{-kx}\, dx$$

where A and B represent constants of integration, which will be received from the property that for $x = o$, $\frac{dy}{dx} = t$, and $y = o$, e represents the basis of the natural logarithms. After having found the values for A and B, we finally receive (in taking the differentials of y) an expression for $\frac{d^2y}{dx}$ containing only known quantities, and in placing said expression equal to $\frac{M}{E\theta}$ we obtain from this last relation the M giving the fiber strain required. The distance x may now again be adjusted in order to find the place where M_{max} occurs, by means of placing $\frac{dy}{dx} = o$. Near the end of the member where the ordinates of the elastic line are small and where the largest moments occur, we may, however, avoid this tiresome way of finding said fibre strain, we take the M received from Equation No. 11 and find our total stress for tension members from

Eq. No. 15: $S_1 = s_1 + \frac{M}{\theta}.y$ where s_1 the allowed unit strain for tension and M, θ and y as in Eq. No. 14.

TABLE OF STRAINS AND SECTIONS OF THE 150 SPAN ALLUDED TO IN THE ABOVE PAPER:
(Compare Fig. No. 6.)

Designation of Member.	Length of Member.	Radius of Gyration.	Number of Pieces.	Shape of Section.	Area.	STRAINS IN MEMBERS. Full Loaded with Live Loads.	By Uniform Dead Loads.	Partially Loaded for Maximum in Web Members.	Maximum Strains.
	Feet.				Sq. in.	Pounds.	Pounds.	Pounds.	Pounds.
$a\ B$	29,15	5,1	2 4	Vertical plates $15''\times\frac{11}{16}''$ Angles at edges of plates $3''\times3''\times\frac{5}{16}''$	27,8	110490	55090	....	165580
$B\ C$	15	5,34	2 4	Vertical plates $15''\times\frac{3}{8}''$ Angles at edges of plates $3''\times3''\times\frac{5}{16}''$	19,15	95910	50400	....	146310
$C\ D$	15	5,15	2 4	Vertical plates $15''\times\frac{19}{32}''$ Angles $3''\times3''\times\frac{5}{16}''$	25,01	125280	66150	....	191430
$D\ E$	15	5,05	2 4	Vertical plates $15''\times\frac{23}{32}''$ Angles $3''\times3''\times\frac{5}{16}''$	28,76	143390	75600	....	218990
$E\ F$	15	5,02	2 4	Vertical plates $15''\times\frac{3}{4}''$ Angles $3''\times3''\times\frac{5}{16}''$	29,7	151630	78750	....	230380
$a\ b\ c$	30	1,44	2	Bars $5''\times\frac{7}{8}''$	8,75	56860	28350	....	85210
$c\ d$	15	1,44	4	Bars $5''\times\frac{3}{4}''$	15,0	95910	50400	....	146310
$d\ e$	15	1,44	2 2	Bars $5''\times\frac{15}{16}''$ Bars $5''\times1''$	19,38	125280	66150	....	191430
$e\ f$	15	1,44	4	Bars $5''\times1\frac{1}{8}''$	22,52	143390	75600	....	218990
$B\ c$	29,15	1,44	2	Bars $5''\times1\frac{5}{16}''$	13,12	75890	42850	87710	130560
$C\ c$	25	3,71	2	10″ channel bar 20.8 lbs.	12,48	48950	26500	58110	84610
$C\ d$	29,15	1,44	2	Bars $5''\times1''$	10,0	57070	30900	67760	98610
$D\ d$	25	2,9	2	8″ channels 15.9 lbs.	9,54	30150	15750	43090	58840
$D\ e$	29,15	1,44	2	Bars $5''\times\frac{11}{16}''$	6,88	35150	18360	50240	68600
$E\ e$	25	2,67	2	7″ channels 10.5 lbs.	6,3	13770	5250	29750	35000
$E\ f$	29,15	0,86	2	Bars $3''\times\frac{11}{16}''$	4,12	16050	6120	34690	40810
$F\ f$	25	2,32	2	6″ channels 7.5 lbs.	4,5	....	....	18040	18040
$F\ g$	.....	.....	.....	....	....	....	....	21030	21030
$G\ g$	.....	.....	.....	....	....	....	....	9130	9130
$G\ h$	.....	.....	.....	....	....	....	....	10650	10650
$B\ b$	.....	.....	.....	....	....	29670	10500	29670	40170

Remark: Since in algebraic expressions very frequently the letter x represents an unknown quantity, and because long expressions ought possibly to be written on one line, the sign of multiplication ×, as frequently

used, has been avoided, and the point used as such in the formulæ of this paper. Consequently in decimal fractions as factors in algebraic expressions the decimal point has been exchanged for the decimal *Koma*, and, for consequences' sake, all decimal fractions have been provided with the *Koma*.

BRIDGE INSPECTION.

By W. S. Thompson.

A paper read before the Engineers' Society of Western Pennsylvania, December 19, 1882.

I have been asked by members of this society to give them some of the results of my experience as an inspector of bridge material. To oblige them I offer this paper, otherwise I would not have trespassed upon your time, for so much has been written upon every department of bridge building and in every style, that I very much doubt my ability to say anything that is new on the subject.

For convenience I will divide the paper into the following heads: Inspectors, their duties, etc.; Drawings, Tests.

INSPECTORS.

Every shop that buys any article, on receiving it gives it to some one to examine to see if it is of the proper material, size and quality. This person, it is clear, must have a knowledge of what this article should be and what deviations are allowable from the order for it. You will reply, of course, he must have such knowledge to be able to decide whether the article is suitable. Perhaps so, but I will venture to say, that if you made such an assertion to the foreman or manager of a bridge shop, he would tell you it might be so in a general way, but that it was not always the case in the appointment of those who were to look after the inspection of bridge material during manufacture. All of them, at least those of the eight or ten shops I have been at recently, as well as some inspectors of my acquaintance, could cite you cases of inspectors who had little or no knowledge of their duties and a very indistinct idea of any sort of manufacture.

What are the requirements for an inspector? As he has to decide questions, to solve which require a knowledge more or less great, of strains, designing, drawing, materials and shop work, it is plain he should be familiar with each of these subjects, particularly the three last, as the two former are generally looked after by his employer. His duties bring him in contact with men of all classes, so I may add that he should have a knowledge of a business way of doing things.

Next, what are his duties? He is appointed by his employer to see for him that an agreement is fairly carried out, not for the purpose of showing his (inspector's) knowledge and authority, nor for the purpose of annoying and delaying the contractor, on the contrary, he should rather assist him if an opportunity offered itself. I do not mean that he is to consider himself in any way an employee of the contractor, but occasions sometimes arise where some such exertion on his part as can reasonably be asked for, will hasten the completion of his employer's work. In such cases it seems to me to be his duty to make that exertion.

At times there will be instances where there is an honest difference of opinion between the inspector and the contractor. Then the former must hear what the latter has to say, but make up his own mind as to what is right, and to hold to that opinion.

Should he think any question that may arise too important for him to decide, he should refer it immediately to his employer and not vascillate nor delay, for by acting in this way he will weary and annoy every one and possibly stop his work.

While any of his work is being done he should make it his duty to be at the shop during the usual office hours, unless he wishes others to decide for him such questions as may arise during his absence. Of course it is not necessary for him to ask permission from the shop authorities when he wishes to be absent, but it is just as well to let them know when and how long he intends to be absent, even if it is only for an afternoon. If nothing else, this is at least a courtesy, and I do not think courtesy is disliked by any one.

The shop is always to supply him with the necessary labor for handling work. So far as my experience goes, there is never any difficulty about this matter, provided he will be reasonable, except that there are times when the labor cannot be given at the moment it is wanted, the exigencies of the shop not always permitting of it; but, as a rule, I have found that the superintendents and foremen were always willing to help an inspector in every way, as long as the latter was not all for himself and thought of no one else. The shop has to pay for the labor they give him, therefore it is only proper that he should, as much as possible, arrange his work and

his time for doing it in a way that will put the shop to as little expense as possible.

He should avoid being distrustful of every one. If he has cause to believe that people are dealing unfairly with him, there are many ways of correcting the matter, or, at least, of relieving himself of the responsibility resulting from such practices.

I have been asked what errors an inspector should look for, &c. It is difficult to answer general questions in a definite manner, but the following may be answer to some I have had asked: Inspection can be superficial or it can be minute, or somewhere between the two. As an instance I may cite the following: An inspector had some 200 pieces about 15′ long and weighing 800 lb. each to examine. He had to make at least four careful measurements on each piece before he could say they were correct. To do this took him about two hours a day for some four or five weeks. Another inspector went over that same lot, and gave it what he called a thorough inspection; the time he devoted to it, all told, was not over twenty minutes.

In examining work it is hardly possible to say what one thing to look for. Properly speaking, however, an inspector should expect to find everything exactly as it ought to be. With many inspectors riveting is the only matter that is looked after, but there are also, and of equal importance, the size and quality of the material of the different parts; the straightness of the finished piece; the accuracy of the work done upon it, such, for instance, as the size and position of the pin or other holes, or slots, and how these will compare with the other parts with which they are to fit. To sum up in a few words, he must see that the work in all essentials is exactly what the drawing calls for; also, that the material and the work done on it is of the quality called for, for any or all of these may be wrong. Now, he can examine one piece for all these requirements; he can examine all pieces for one thing, as, say riveting; he can see that each piece is correct in every detail; or, as I have seen done, he may be able to cast a glance over a lot of work and say whether it is right or wrong. Of how this was done I am ignorant, but of one thing I am sure, the opinion as to its condition was not worth much.

There are times when some points in examination may be omitted; judgment and experience must decide when this can be done, but there should be no mere trusting to luck.

The manufacturer is most undoubtedly responsible for all these things. Inspection does not relieve him from responsibility, nor does his responsibility relieve the inspector. The latter's employer, and the manufacturer also, provided the inspector is competent, think the matter is of so much importance that it is well to have some one to see that the agreement is fairly carried out.

It might not be amiss for the inspector to remember "that he has a reputation to sustain," and that if he is careless, others will likely be so too; also, that in the oft quoted Tay disaster there was a strong presumptive evidence that the inspectors had neglected their duties, or were ignorant of them.

What allowance can be made in the way of deviation from drawings? For definite answers special cases would here also have to be cited, but generally the following may be made: As to size shape irons are generally accepted if they are not more than 3 per cent light. Rods, flat bars, and plates are more easily rolled to a size, therefore they are seldom accepted if not up to that size, though under special circumstances if the light pieces are but comparatively a small proportion of the whole number, the above percentage may be allowed.

Riveted members should be pefectly straight except top chords and inclined posts, which may perhaps be better for having a slight camber, but I often find it necessary to accept posts say 30′ long out of line by ¼″. Some few men work closer than this, and it must be acknowledged it is a very vital point. I inspected the material for a very large bridge, every riveted member of which, except one, was perfectly straight; and the error in that one was, if I recollect aright, ¼″ in 35′.

Punching is not always accurate. If it is not out more than ¼″ in 20′ or 30′ it is considered tolerably good work. In many cases angles used as brackets, if, after being riveted on, they are within ¼″ of position, it is considered correct. This is usually near enough.

When work of the above kind must be more exact, it should be so stated, as it calls for an extra class of work. In the width of chords, i. e., the distance apart of side plates or channels, it is usually considered very fair work when they are within 1-16″ of the distance called for, though they must be nearer than this when the side plates are thin, say ¼″.

Top chords and posts should never have their pin holes bored less than the distance apart called for. 1-32″ excess in former and 1-16″ in latter will never do harm, except in some few special designs. Bottom chord and tie bars should, if they vary at all, be less than the length called for. 1-32″ error in these will not be at all serious provided all the bars are bored to exactly the same length. Struts if bored within 1-16″ are near enough.

Shop foremen generally follow the above rules.

In making eye-bars, the smith is allowed ⅜″ over or under length from out to out. In thickness of head he is allowed 1-32″ under or 1-16″ over required thickness, which he

sometimes exceeds, but nothing is allowed at the neck, as this is apt to be the weakest part of the bar. In making lateral or other similar rods he is allowed to vary ½″ either way from required length. Other parts must be made closer than this, but smiths rarely forge very closely.

Pins should never be taken if shorter than called for unless their lengths are calculated for washers, and they should be examined closely to see if they are of uniform diameter and not gouged out at the corners or ends.

Rollers may vary a little from required diameter, but they must be all of the same diameter.

Web plates of girders of all classes should be flat, but it is customary to allow the web of large girders to be slightly buckled or dished to the amount of say ⅜″ versed sine to a 6′ chord.

Mills do not always deliver plates perfectly straight. Have them straightened if necessary, but otherwise, when possible, use them as they are, for there is no way of straightening them except by the sledge or drop hammer neither of which are very good for the iron. For other similar matters I can only refer to common sense and that intuitive knowledge which is the result of experience.

As to inspection at mill. An inspector can but seldem see the actual rolling of his material unless the mill folks would roll only to suit his convenience, and even then it would likely be impracticable.

Tension and bending tests of the material can be made at the mill, though where it is practicable, it is very much better to cut off and test the test pieces at the shop where the material is delivered; this can easily be done by ordering extra pieces or a few pieces of extra length. The inspection of the rolled iron, however, is best done at the shop, for there one can usually see it to better advantage, to say nothing of the fact that the most serious flaws are generally made apparent during manufacture.

My experience has been that the inspection of material at mill, except testing, generally amounts to nothing, for often times they have to load material (at which time the examination is expected to be made) when the inspector cannot be there, or after dark when he cannot see; this is not from any desire to be contrary, for though they are generally hard worked I have found shipping clerks very accommodating, but at an iron mill it requires them to use their wits to keep the yard clear.

Inspectors necessarily lose a great deal of time waiting on others. This is of daily occurrence and cannot be avoided, and such time, as a rule, has to be a dead loss.

I have been asked what are the faults of some of the bridge shops. To answer that I fear might be considered as getting personal, though undoubtedly to my thinking all of them have their faults, as, with a few exceptions, they all have one fault in common, perhaps I can speak of that, viz., painting. A bridge shop painting gang usually consists of a lot of half grown boys led by a man who has not a very great reputation for either force or thoroughness. The chief idea of the whole crowd is to see how fast they can get over work. It seems to be of no consequence to them whether the whole piece is painted or not, for they sometimes seem to think that it is unnecessary to paint corners or unseen parts so long as the piece looks like it was painted. Possibly this may be a result of their usually being hurried, to say nothing of bridge shop paint work having to be done out of doors and in all sorts of weather. I have no doubt you will smile but I can assure you that one of the most satisfactory painting gangs I ever came across was led by a boy fourteen years old, most satisfactory because the boy was a good foreman and saw that his gang did their work faithfully.

DRAWINGS.

The inspector seldom has anything to do with the making of drawings, but he has a great deal to do with them after they are made, and this generally under unfavorable circumstances, consequently he is much interested in their clearness and fullness of detail.

The bridge draughtsman should bear in mind that drawings are a species of picture-writing, used to convey to those employed in shops a clear idea of the wishes and intentions of those in charge of designing. Some individuals when writing a letter will write a good, clear, readable hand without crossing their lines or any fantastic additions to their letters; they also express themselves fully and clearly, consequently one can read their letters rapidly yet have a complete idea of their contents. Others write a cramped illegible band, add fantastic scrolls to their letters, omit important words and fail generally to convey their ideas clearly. With writers of this latter style all of us know what a time it requires to find out the exact import of their letter. It is the same with drawings. Some men make their drawings so clear that they can be read at sight; others again draw in such a manner that in attempting to read their drawings you fancy that they tried to make them as much like puzzles as possible. The draughtsmen should also bear in mind that there is a difference between the style of a drawing to be made for shop use and one that is made for a periodical or other similar purpose; also that when he uses a drawing he has clean hands, a dry, quiet, and well-lighted place to lay his drawing and weights to keep the corners down, and paper and pencil to supply missing data; but not unfrequently the contrary of this is the case

with those in the shop, and particularly so with inspectors, who often times have to hold drawings in high winds and at the same time use rule and callipers, &c. In other words, he should not make sheets too large, nor crowd too much on one sheet, nor put so many lines on top of each other that it is difficult to follow any, nor omit any figures that are required to be known at a given point because they can be found on bill of material or another drawing; thus necessitating a bill of material and a couple of drawings to know, say, what a post end should be, and he should not imagine that pieces of unfaced material are always the exact length called for.

Some may say that the draughtsman's time is expensive, and the foreman would understand this or that. True, the foreman generally can do so, but the foreman does not do the work nor stand over the men doing it, consequently he has to supply the omission of this picture writing to his men, i. e., do draughtsman's work to the neglect of his own, which, according to an old-fashioned notion, is that he should look after the quality of the material being used, what sort of work the men were doing, and matters of similar kind, all of which was thought enough for him to do. This latter may be an old-fashioned custom, but I believe it is still adhered to in locomotive and other machine building shops.

Foremen do not always check the drawings to see if they are full and complete; unfortunately this results some times in work being spoiled, simply because the drawing did not state what was wanted. True, the foreman could have seen what the draughtsman had omitted if he had been doing the work himself, but his men do not necessarily have the same education and ability as he has, and drawings are as much for the men's use as any one else.

The errors of draughtsmen, who can be called such, are not, as a rule, in the large matters, but in the small ones, which they think are unimportant and can be left to the shop. This is a fruitful cause of delay and expense, generally small, though not always so, and, in my experience, is one of almost daily occurrence; in fact, I doubt whether I have had a single span under my charge for years on which there could not have been saved from a few dollars upwards by the draughtsmen paying a little more attention to clearness and those little things which every one knows.

Great accuracy is unnecessary in some of the parts of a bridge which it will be well to remember, as the more accurate the work is required to be the more expensive it will prove, also the fewer the parts the cheaper will be the work. Short pieces of angle iron or any small pieces fastened to a member weigh little but cost greatly.

A drawing should be clear and to the scale, with little or no shading, and when possible should be complete in itself without reference to anything else. Time saved in the drawing room is not always an economy. In making a drawing a draughtsman should use as few different sizes and lengths of material as possible, for similarity in sizes of pins, bars, rods, and in lengths generally, saves in the cost of workmanship and reduces the chances of error. A theoretical saving of material sometimes necessitates an outlay of several times the amount saved to do the extra work, also usually each different form necessitates a different set of rolls for its making, which may cause delay in delivery of material. The designer should have some consideration for what is obtainable, as for instance, a variation of $\frac{1}{2}$ lb. per foot in a 12″ channel only makes a difference in thickness of web of 1-100″ closer than it is possible to roll, unless there are a large number of same weight.

Plates and bars ordered by 1-32″ or squares and rounds ordered by 1-16″ generally come either full or scant, more likely the former. 1-32″ is looked upon in bridge shops as somewhat of an imaginary quantity, and particularly so to ask it of a blacksmith. My own experience has been that by those working on tools it is considered to mean scant or full of a given 1-16″, but a smith scarcely seems to realize what it is. Tell him a piece is to be so many sixteenths thick and he will come very near it, but ask him to make it so many thirty-seconds thick then you will be lucky if you get it within an eighth. The office says the man should be educated up to this close work, to which the shop replies that the office should be educated up to their wants. I say, that if the office wants such close work, the men are to be found, but they are only to be found in Government arsenals and tool making and other establishments that require very close work.

Of late days it seems to be considered important only to be very careful to give distances of rivet spacing. This is no doubt of prime necessity to the layer-off, but not so to the machinist and inspector, the former only wants to know the distances he has to work to, and the one governing point from which he has to start; besides this the inspector usually only cares to know if the number of rivets is correct, and that their spacing is approximately so. In making drawings, distances from center lines and other governing points should be given, and all parts that are to be planed, bored, or drilled, should be so marked. A hole or slot should always be marked with the size of the pin or tenon that it is to take; and stating what sort of fit is required. Angles are best defined by their base and perpendicular, and it should be remembered that sheared parts are not perfectly straight or smooth; that a punched hole has not parallel sides nor is it always

exactly where it ought to be, and that when two or more plates are riveted together their united thickness is generally greater than the sum of their several thicknesses; that right and left pieces or other complications are to be avoided when possible, as they are troublesome and liable to cause errors; and that rivet holes should not be too large a proportion of the width of a channel flange, nor too near its edge.

As few rivets should be left to be driven in the field as possible, for they are more expensive and less reliable than those driven in the shop. As to the latter, shop hands have every convenience possible and are daily occupied in driving rivets, whereas the men in the field have on the other hand almost every inconvenience to contend with, and driving rivets is but a very occasional part of their work.

TESTS.

Looking over the results of a large number of specimen tests of wrought iron made by myself, I find that the results are about the same for bars, rods, plates (edge rolled), angles and channel bars, viz., an ultimate strength of from 50,000 to 53,000 lb. per square inch, generally 51,000, and an elongation in 6″ of from 16 per cent to 25 per cent, channel bar webs, and plates ranging chiefly from 16 to 20 per cent. This I think is very much due to the fact that in the test pieces made from these it was necessary to use a rectangular section. I have tested sheared plates up to 72″x$\frac{3}{8}$″ with the same ultimate strength as the above, but the elongation varying from 9 per cent to 16 per cent.

Bending Tests.—Bars, rods, and channel flanges with few exceptions bent until flat or their ends touched with a curvature whose diameter varied from $\frac{1}{2}$ to 3 times thickness of piece, generally $1\frac{1}{2}$. Plates and channel webs would bend 150 degrees or until their ends touched with a curve whose diameter ranged from one to four times thickness. All bending pieces were about one foot long and from 2″ to 3″ wide. Here it may be well to say that bending pieces must not be sheared, but planed, and also have their edges well rounded on a grindstone or emery wheel, so as to remove any incipient cracks.

These are tests of such wrought iron, with but few exceptions, as has been used in the work that I have had charge of during the last six years. Of course all will understand that these tests were applied in line of fibre, as is the ordinary usage* Where more than this is expected, viz., a strain crosswise to fibre, it is always specially mentioned, as witness British Admiralty and other specifications. The reduction of area has been omitted, as the measurements taken for it are generally unreliable, and as Stoney says, the elongation is much more reliable for any question as to quality of iron.

*See table for transverse strength of plates.

I have other tests, which are of condemned lots, but presume they would be of no interest, for surely no one is going to try for the worst material he can get. I have had but little to do with cast iron since testing has become common, but have about 12 or 15 tests from as many heats. In these, the bars 1″ sq., 5′ long, 4′ 6″ between supports, it took from 650 to 700 lb. resting on dull knife edge at centre to break them, which was a better result than asked for. Of course the foundry hands took good care to place the bar on supports same side up as when cast. Such cast iron should only be used in very special cases, for it is certainly much too good to be used in such places as masonry plates, and washers of bridges.

Thus far I have only tested steel from plates rolled in this country from English Bessemer blooms. I made a large number of tests from these plates, both tension and bending; of the latter there were over 100. The plates ranged in size from 12″x$\frac{1}{4}$″ to 24″ x$\frac{3}{4}$″. The results were very uniform, giving an ultimate strength averaging 70,000 lb. per square inch, with an average elongation of 30 per cent in 6″. The bending pieces were planed from plate shearings very little broader than the test piece. All these were well rounded on grindstone, heated red hot, and dropped into water of a temperature from 70 to 100 degrees. Nearly all bent 180 degrees flat, some few of those $\frac{1}{2}$″ to $\frac{3}{4}$″ cracked at edges when 90 degrees; these latter were then punched within $\frac{1}{4}$″ of edges without cracking, probably there were invisible edge cracks.

Some of you no doubt would like to know what strain full size pieces, just as they go into a bridge, will stand. I have enough of one kind I think to satisfy you, viz., of tension members. I have made a few tests of compression pieces which have been published elsewhere.

I will first give you the result of four lots of bars ranging from 1″ to 2″ diameter, and from 15′ to 25′ long with enlarged ends for thread.

1st lot consisted of 60 iron rods. 14 of these broke in thread, the rest in the body of the bars with 12 to 19 per cent elongation. All except four stood over 45,000 lb. per square inch; one of these broke at 35,000 lb., two at 40,000 lb., and one at 43,000 lb. per square inch.

2d lot; 23 rods. 13 broke in screw, the others elongating 12 to 16 per cent. All but one stood over 45,000 lb.; that one stood 44,000.

The above are only moderately good.

3d lot; 38 rods. 11 broke in screw, the others elongating 13 to 18 per cent. All but

seven stood 50,000 lb. or over; these broke as follows:

1—38,000	lb.	per	square	inch.
1—42,000	"	"	"	"
1—43,000	"	"	"	"
1—48,000	"	"	"	"
3—49,000	"	"	"	"

4th lot; 30 bars. Two broke in screw. All stood 50,000 lb. per square inch or over. Elongation of those that broke in bar was from 10 to 18 per cent, generally 16 per cent; nearly all these broke within 2 ft. of end of rod as did those in the other lots.

Another lot of 20—$1\frac{3}{8}''$ round rods with $1\frac{3}{8}''$ threads (all of course in thread) broke with 37,000 to 40,000 lb. per square inch of rod.

Before proceeding further I may as well state here that the above lots of rods were made by different shops, as were also the eyebars, the test results of which I am about to give you. These I have classed by width of bar, as it will give you as good an idea of the average eyebar as any other classification.

I will first give you the results of tests of some heads made entirely by upsetting, merely saying that they have been given to me and that I did not see the tests made:

Of 52 bars about $3\frac{1}{2}''$ wide, 22 broke in bar elongating 7 to 15 per cent, and 30 broke in the head. The ultimate breaking strains seemed too high, consequently I omit them excepting to say that one broke in the head at 36,000 lb. per square inch and two others at 42,500 lb.

Next I will give you results of tests of eyebars made by piling, with the exception of some 10 or 12 bars the iron in all of them showed a good fibrous fracture and with very few exceptions, perhaps six, the heads were proportioned by usual formula.

Of 26 6″ bars 13 broke in bar with following strains:

5 between 40,000 and 45,000 lb. per square inch.
6 " 45,000 and 47,000 " " " "
1 " 47,000 and 49,000 " " " "
1 " 49,000 & upwards " " " "

13 broke in head.

11 between 42,000 and 45,000 lb. per square inch.
2 " 45,000 and 48,000 " " " "

Of 159 5″ bars 80 broke in bar:

2 between 35,000 and 40,000 lb. per square inch.
10 " 40,000 and 45,000 " " " "
17 " 45,000 and 47,000 " " " "
27 " 47,000 and 49,000 " " " "
24 " 49,000 & upwards " " " "

79 broke in head—one at 33,000 lb.

6 between 35,000 and 40,000 lb. per square inch.
36 " 40,000 and 45,000 " " " "
22 " 45,000 and 47,000 " " " "
11 " 47,000 and 49,000 " " " "
3 " 49,000 & upwards " " " "

Of 46 4″ bars 26 broke in bar:

3 between 40,000 and 45,000 lb. per square inch.
11 " 47,000 and 49,000 " " " "
12 " 49,000 & upwards " " " "

18 broke in head—1 at 37,000 lb.

10 between 40,000 and 45,000 lb. per square inch.
1 " 45,000 and 47,000 " " " "
4 " 47,000 and 49,000 " " " "
2 " 49,000 & upwards " " " "

Of 19 3″ bars 14 broke in bar:

5 between 45,000 and 47,000 lb. per square inch.
3 " 47,000 and 49,000 " " " "
6 " 49,000 & upwards " " " "

5 broke in head:

1 between 40,000 and 45,000 lb. per square inch.
2 " 47,000 and 49,000 " " " "
2 " 49,000 & upwards " " " "

From the foregoing it seems to me that it should be called good results, but having the data, you can judge as well as I, when

6″ bars break at 45,000 lb. and upwards.
5″ " " " 46,000 " " "
4″ " " " 47,000 " " "
3″ " " " 48,000 " " "

With an elongation of not less than 10 per cent nor more than 25, to be measured in not less than 5′. Bars of same rolling even when broken in the body will give differences in elongation; 14 to 16 per cent, however, is the most common elongation in the above tests. They will also break at different strains; e. g. $6''\text{x}\frac{3}{4}''$ bar broke at 46,800 lb. per square inch, elongating 12 per cent when a $6''\text{x}1\frac{1}{8}'$ bar broke at 49,500 lb., elongating 15 per cent. One $3\text{x}1\frac{1}{8}''$ bar broke at 46,000 lb. per square inch, elongating 17 per cent, another $3\text{x}1\frac{1}{8}''$ bar broke at 50,000 lb., elongating 17 per cent, all of which bars I believe were rolled from exactly the same stock and seemed to me to be purely fibrous and without flaw. All who have done much testing know that results will sometimes vary in test pieces cut from same piece of iron, to wit, a $\frac{3}{4}''$ round; but the above variations seem to be greater than is due to this cause. Some say it is the result of unknown factors in the manufacture; perhaps the effect of heating only a portion of the bar. Is it so?

Some tests made at Watertown on 5″ and 3″ flats 10′ long showed even better than this, but all those bars were rolled specially for the test, had no rough handling, and were not heated or worked on after leaving the rolls. From these and other tests it seems probable that all double-rolled iron bars will, if tested as they leave the rolls, stand from 50,000 to 52,000 lb. per square inch ultimate strength with an elongation of 15 to 23 per cent.

Some lay stress upon having a bar break in the shank. I do not think it is so essential, though I prefer it, for when the bars have elongated 8 per cent the bottom chords and ties will have lengthened so much that the bridge will most likely fall between its abutments.

I append a table of some tests of eyebars, but wish to give results of three here:

$5''\text{x}1\frac{3}{8}''$ bars had an elastic limit of 27,000 with an ultimate strength of 37,800, broke 6′ from pin, little or no weld. Moral rolling mills do not always pile full length piles. I

am very sorry to say I could show others like this, and testing to destruction is often the only way of discovering this defect.

A 5″x1⅛″ bar 12′ centers had an elastic limit of 33,000 lb., but broke with 33,500 lb.

A 5″x1 7-16″ bar 15′ 3″ center pins 4⅜″, had accidentally been strained beyond its elastic limit, being lengthened thereby ¼″, pin holes had a slight set say 1-100″. Elastic limit supposed to have taken place at 31,000 lb. This bar was lent to me providing I did not stretch it any more. I put the bar into press with 4⅛″ pins and applied a strain of 15,000 lb. per square inch, holding it 5 minutes, released this and applied a strain of 20,000 lb. which was likewise held 5 minutes and released. Finally a strain of 24,500 lb. was applied for 5 minutes. The pin-holes were then measured; both measured the same, viz., 4 25-64″ transverse to bar, and 4 13-32 in line of bar.

The test of 5″x1⅜″ bar is given to show that rolling mills are not always careful, even when paid for extra good iron. That of 5″x 1 7-16″ bar is given to show that with properly proportioned heads it is not necessary to make pins fit holes so very accurately as is often times required. I do not, however, advocate such a difference as this between pins and pin-holes, but I do not think that it is allowable to make pin-holes 1 32″ larger than pins for diameters of 4, 4½″, and upwards.

Neither 5″x1⅜ nor 5″x1⅛″ bars gave any intimation of their yielding until their elastic limit had been very nearly reached.

By some great stress is laid upon applying a proof strain. I have personally tested several thousand bars and rods of all styles of make, but never found one defect by this proof strain. Indeed, I have subjected eyebars with visible defects to this strain, and although I have closely watched them nothing more could be seen than before the bar was under stress.

I have found proof strains serviceable only with small things that were considered of no importance by the smith.

Whenever the matter is left to me I never use a proof-strain except where the ends are welded to main body of bar or rod, *i. e.* finished bar longer than original bar, and then not that I expect it to be of any service, but because I considered it a safe plan to examine a weld in every possible way. In place of proof-strains I should advise in addition to this, that a number of extra tension pieces should be ordered and when the whole lot are finished, choose from the lot as many as you have ordered extra and then break them, but not till all are finished. The cost will not be great and I am sure it will have a good effect, for having tried it on a small scale I have found it to work wonderfully well. So I may say I speak to some extent from experience.

Some railway companies require the modulus of each bar to be taken. In my experience this cannot be done with any accuracy with such appliances as are to be found in shops. To be accurate the bar ought to be counterbalanced at say every ten feet with movable weights so as to eliminate the effects of any sagging which is produced by weight. The pressure gauges should read accurately and not approximately, and much more time given to it than ordinary shop testing will admit of.

I have tested over 5000 bars for their moduli, and all the results I have obtained lie between a moduli of 24,000,000 and 30,000,000, or say extensions of .008″ to .01″ per ft. for 20,000 lb. strain.

Generally at any one testing the moduli did not vary more than 3,000,000, of which the equivalent variation in extension would be .001″ per ft.*

In my opinion even this is greater than it would be if all sources of error were eliminated, for I am inclined to believe, on looking over the tests I have made, that the moduli of the iron (from three different mills) I have tested, lies between 26,000,000 to 28,000,000 or the equivalent extensions of from .0092″ to .0086″ per ft. to 20,000 lb., and that the extremes are perhaps even less than here given.

Since writing the foregoing I find that in Watertown-tests of 25-10′ bars two different classes of iron from two different mills making the lot referred to above, the extension of 21 for 20,000 lb strain was within the above limit; the others had the following: .0093″, .0084″, and .008″ per ft. I feel inclined to think that this last ought to be omitted. Taking them as they are for 10,000 lb strain per square inch, the variations in bars 30′ long would be .021″ or one and a half sixty-fourths of an inch. In six bars rolled at one mill the difference of extension for 20,000 lb. strain, was .00048″ per ft., which would be for a 30′ bar with 10,000 lb. strain, 1-128″ nearly.

As so much testing of bars has been done, perhaps it would be well to state the different ways of making iron eyebars. As to design, the heads may be square, octagonal, circular, pear shaped, or an oval made by striking the curves forming eye from two centres with same radius and connecting them by tangents; this is done to give a greater proportion of metal back of eye, which, I think, is very judicious, because it makes some allowance for the inaccuracy of the smith, and experience convinces me that it aids very greatly in preventing any deformation to the eye; for when so made, and other proportions as

*These elongations for moduli were taken between strains of 10,000 lb and 20,000 lb per sq.″; previous to adopting this method the elongations were taken between strains of zero to 20,000 lb per sq.″ and I then obtained as great variations as you can find published for the unsupported length of bar was the greatest factor in the result.]

usual, it is extremely rare for them to break at pin-hole. Old English tests, and also those of Chief Engineer Sprague, of the navy, confirm this. Generally the third and last shaped heads are used; they may be either same or greater thickness than the bar, and are connected to the bar by the neck, which is formed of curves of greater or less radii.

As to manufacture:

1st. These heads may be cut out of a rolled plate or may be forged from billets, and welded to the bar.

2d. The body of the bar may be upset to form the head without the addition of any extra metal.

3d. Piling pieces are put on end of bar and the whole partially upset, after which they are pressed or hammered into shape in a die.

4th. Same as last, omitting upsetting.

5th. Is the Springer patent. This is made by bending a piece of iron of proper size and same thickness as a bar requiring head into somewhat the shape exteriorly of a horse shoe, with a rectangular slot on the inside. This is slipped on to end of bar, and cover plates cut to shape placed on top and bottom. The whole is then heated and pressed or hammered into a die, as above.

6th. Is the Kloman bar with which you are all acquainted.

7th. Is the loop eye which is formed by bending the bar edgewise around a pin and welding edges together.

No. 1, or welded bars, are now-a-days considered inadmissible.

No. 2, or upset bar, makes a very good bar and the manufacture of it will probably be improved, but I am inclined to think it would be better if they used a little piling.

Nos. 3 and 4, or piled head, is the one in most general use, and makes a very good head, but care should be taken to keep the diameter of the eye and radius of neck as small as possible, the dfficulty of manufacture increasing with diameter of eye, and the bar should not be too thin to hold its heat nor should the head be piled crosswise.

5th. The Springer bar, gives any desired latitude to size of head. All bars of this make that I have seen tested broke entirely clear of the head, except one, which broke at pin hole at 49,600 lb., the head having an excess of only 31 per cent. I have broken four heads longitudinally and transversely, all of which showed solid welding.

6th or Kloman. This makes a very good bar, but the only tests I have of these were made on bars Mr. Kloman tested for his private use.

The 7th if made by a good smith will, I believe, always break in body of bar.

For steel bars heads are designed after same pattern as those of iron, but with them welding or piling is not permissible, so that either a Kloman head or one made by upsetting is here necessary. The Edge Moor Iron Co. has a plant for upsetting steel bars. At the time I examined it I was fully convinced that the process could not fail to make a good head; I have since been told that all that had been tested broke near centre of bar when properly annealed; *i. e.* whole length at one time; if only annealed at ends they broke near where effects of annealing ceased.

The annealing of steel eyebars appears from tests to be a matter of considerable importance.

As to round and square rods, they have eyes and enlarged ends for screws made after the same modes as eyebars except that the diameter of the ends of iron bars are sometimes increased by piling or splitting the bar, putting in a wedge, and welding all up

I intended to have embraced specifications as a part of my subject, but I have detained you long enough. I hope some one will give us a paper on this subject and in it show that some specifications ask for impossibilities; that others have costly manufacturing requirements which are omitted after the work is let, and that much also could be done to relieve the labor of estimating by adopting a standard column formula and by adopting rolling loads at so much per foot or, at least, using standard engines.

Results of Tests of Eye Bars with Heads made with Piling Pieces—Tests made with Hydraulic Machines fitted with Mercury Gauges. Bars varied in Length from 10′-0″ to 25′-0″.

Size of Bar in Inches.	Elastic Limit in Pounds per sq. Inch.	Breaking Strain in Pounds per sq. Inch.	Per cent of Elongation.	Distance of Fracture from nearest Pin Centre in Inches.	REMARKS.
6x1⅝	24.500	43,700	11	—	Broke at head.
6x ¾	30,000	47,000	13	—	" " "
6x1	27.000	40 500	5	12	Granular.
6x1	27,000	42.200	7	24	"
6x1 9-16	25.600	45,800	21	32	Fibrous.
6x1	29.900	46,500	13	17	"
6x ¾	30,600	46.800	12	15	"
6x1 3-16	28,800	47,400	15	32	"
6x1⅛	28,000	49 500	15	21	"
5x1⅛	28.300	43.500	10	—	Broke at head.
5x1	31.000	46 000	8.5	—	" " "
5x1	28,0 0	47.000	9	—	" " "
5x1 5-16	29 000	48,700	11.5	—	" " "
5x1 7-16	25 800	37.000	4	24	Granular.
5x1⅜	26 500	37,800	5	66	Fibrous. Bar not welded.
5x1	31,000	42.000	4	—	In bar, distance and character of fracture not noted.
5x1	28,600	45.000	14	85	Fibrous.
5x1⅜	28,000	46,500	9.5	24	"
5x1	29.000	49,000	16	24	"
5x1⅛	30.000	49.000	14	20	"
5x1	30,000	50.000	16	20	"
5x1¼	30,000	50.000	16	20	"
5x13-16	30.400	51.000	15.2	60	"
5x1	31.000	51,000	15	136	"
4x1¼	28.000	43.000	10	—	Broke at head.
4x1 1-16	30.000	48.600	12	—	" " "
4x1⅛	29 000	45,000	13	80	Fibrous.
4x1 5-16	27.300	47,500	18	21	"
4x1¼	29,000	47,800	15 5	26	"
4x ⅞	34,000	48,000	6 8	24	Granular.
4x1¼	27.400	48.000	13 3	16	Fibrous.
4x1⅛	28.300	48.500	16 6	18	"
4x1 3-16	28.700	48.700	14	20	"
4x1	30.500	49,200	13	12	"
4x1¼	30 000	49.600	16	13	"
4x ⅞	32.000	49,800	15	24	"
4x ⅞	29,7 0	50,000	16	2	"
4x1	31.800	50,000	15	—	" Broke in bar position of fracture not noted.
3x1⅛	30,000	46.000	17	24	"
3x ⅞	30,600	46 400	14	24	"
3x1	—	50,000	12	—	" Broke in bar position of fracture not noted.
3x1⅛	31.700	50.000	17	22	"
3x1¼	32,000	50,000	15.3	22	"

Results of Tests of Iron Plotes—Tension Tests made with Lever Machines.

	Lengthway.					Crossways.			
	Thickness in inches.	Ultimate strength per square inch in pounds.	Per cent elongation.	Elongation measured in inches length.	BENDING.	Ultimate strength per square inch in pounds.	Per cent elongation.	Elongation measured in inches length.	BENDING.
A	¼	52.000	20	6	180° flat.	29.500	2.2	4	20° to 3" circle.
"	⅜	50,500	19	6	180° flat.	31,400	1 5	4	25° to 9″ circle.
"	⅜	51,000	24	6		34,300			
"	½	51,000	21	6	180° to 1¾″ circle.	31 500	3	4	25° to 9" circle.
"	½	52,000	23	6		33,400	2.5	4	
"	⅝	51,000	17	5	Ends touching to ⅝″ circle.	28,900	2	5	15° to 4″ circle. 25° to 9" circle.
B	⅜	50,700	9.4	8	Ends touching to 1″ circle.	36 500	2	3	
"	⅜	51,000	9.5	8	" " to 1½″ circle.	36.000	...	...	30° to 3″ circle.
"	⅜	50.600	13	6					60° to 5″ circle.
"	⅜	51,600	13	6					
"	⅜	51,700	17	6					
"	7-16	54,000	...	...	160° to 2″ circle.	39.000	2.3	3	
"	7-16	50,500	9.2	8	170° to 2″ "	34,400	2	3	40° to 6″ circle.
"	7-16	51,500	12 7	8	170° te 1″ "	38,000	3	3	
B	⅜	46.400	13	8	180° to 1″ circle.	31 800	0.6	8	
"	½	50,800	11.7	8	180° to 2″ "	32,600	1	8	
C	¼	53,700	7	10		not	given		
"	½	49 300	6.7	10					
D	½	47.500	9	10		42 500	2.7	10	
"	¾	47,900	10	10		41,900	3.8	10	
E	⅜ to ⅝	47,700	16.7	10		45,500	11.2	10	

Tests A were from American edge rollod bridge plate.
" B " " " sheared " "
" C " " ordinary English ship piates.
" D " " " " boiler plates.
" E " " Yorkshire wrought iron plates.
Tests C D & E are a mean of several tests made by Mr. Kirkaldy.

ANNUAL REPORTS OF OFFICERS.

JANUARY, 17th, 1883.

ADDRESS OF THE PRESIDENT.

To the Members of the Engineers' Society of Western Pennsylvania.

GENTLEMEN: The third year of the existence of our Society closes with this meeting. The Engineers' Society of Western Pennsylvania has passed the period of problematical existence and doubtful vitality and has developed into the strength and self-reliance of manhood.

There is no room for doubt in our minds to-night, but that the work begun three years ago will be carried to its final realization. This single fact in itself, without relating in detail the achievements of our strifes and labors, is sufficient cause to congratulate ourselves and to strengthen our faith unto the prosperity and usefulness of our Society for the future.

A corporals guard met three years ago to try, for the last time perhaps, to organize an engineers' society in this city and district; an enterprise which had failed repeatedly.

The few who met at that time were convinced that our city had the material to form a first-class society, and all were unanimous that no other city had more direct interest in the existence, development and growth of such a society, than our Smoky City, with its vast manufacturing interests, employing engineering talent in all its branches.

Heterogeneous as our material was, the objects of our meetings undefined, the process of crystalization attracted and united the molecules to a definite regular shape, and we now meet as friends amongst friends, ready to learn of and to impart our experience to others; each one of us awaiting anxiously the regular evenings of our meetings, which are always well attended, far better than in other similar societies.

An average attendance of forty four, or almost twenty per cent of the total membership, and this latter steadily increasing, is certainly the best proof of the interest taken by our members in the work, and for the accomplishment of the objects of our Society.

Seven papers were read the past year in the nine meetings of our Society, and printed copies furnished to each member. It is an unexampled achievement, that with the limited means at our disposal, the modest regular annual dues being our only income, we not only managed to defray all the necessary expenses of the Society, but also to have all the papers read during the last three years, with the necessary illustrations reprinted in a respectable book form at an expense of nearly six hundred dollars, and to furnish a copy free of charge to each of our members. Most of the papers contained in that volume are of considerable value, not only to our members, but to the profession generally, and will secure to the book a warm welcome in every library.

To our members of this date, however, it will be in future days like an old dear friend, reminding us of our early efforts and representing the first tangible fruit of the Engineers' Society of Western Pennsylvania. The number of volumes of our library has been increased by one hundred and ten, making a total of five hundred and ninety-five volumes, selected carefully from the best authors of all countries, and forming a library of reference of a high standard. The funds donated to our Society for our library by two of our liberal minded citizens, one of whom is our honored former ex-president, have been expended for these books, and the binding of the same. I know that I speak your sentiments when I again express the sincere thanks and gratitude of our Society to these generous gentlemen, for the aid they have rendered us by their donations.

I feel it to be also my pleasant duty to render the thanks of our Society as well as of myself to the board of directors, to the treasurer and to the library committee for their close attention to our business and for the untiring efforts on their part to improve and perfect the standing of our Society.

Our Society is under special obligations to our librarian for his excellent services in attending to the perfection of our library, and for his great painstaking in compiling the first complete index of our books with their full titles. Each member will receive a printed catalogue of our books, which will greatly facilitate the use of our library.

Our thanks are also due to our secretary for his faithful services in keeping our records, and attending to the important but laborious routine business.

On my own behalf I beg to express thanks to the members of our Society for the uniform courtesy and kindness I have been treated with, and to ask their pardon for any shortcomings on my part, as I have tried to do my duty as best I could.

The papers read before our Society are creditable productions, and bear testimony that we have amongst our members engineering talent and experience equal to that of any other similar society, and the discussions of the papers in our meetings prove that our members are alive to understand the importance of that part of our proceedings. In fact the thorough discussion of subjects in our meetings is more desirable and more important for our improvement and instruction than the reading of many papers, as the different views brought out are bound to elucidate the subjects from different standpoints and under different assumptions, than the writer of the paper may have chosen, bring out the varied experiences of other members under similar or different conditions, and call our attention to factors which we either did not know before, or which we had overlooked, but which we ought to know in the execution of engineering work, in order to either make use of or to guard against them. The more animated our discussions become the better will it be for our Society, the more frequented will be our meetings, and so the information obtained will be imparted to a greater number, for the benefit of a still greater one. If in this respect we have not yet reached the climax of perfection, you must not seek the reason for it in the indifference or incapability of our members, but quite in a different direction.

Engineers are, and naturally must be, of a deliberative turn of mind; they are engaged in work which requires deep and steady studies, oftentimes compelling them to work for days and weeks to solve a problem.

With paper, teesquare and triangle before him, a pencil or compass in his hand, in a quiet room, or some secluded corner of a shop, the engineer concentrates all his faculties on the subject before him. Words and speeches are rather a disturbance to him, the less he hears of them, the more comfortable does he feel.

It is but natural that under such circumstances you will not look for oratorical ability amongst our class. If you bring such an engineer into a large hall filled with people more or less strangers to him, he feels embarrassed and awkward when he ought to speak, and he will rather sit still and listen to remarks of others, which he could flatly contradict with a few words, if he could muster courage enough to get up from his chair at the proper moment, thereby not only doing injustice to himself, but also causing positive harm to the members; as sometimes erroneous statements, not contradicted by anyone at a large meeting, are by some accepted as established facts, to be guided by. After one of our meetings, where an interesting paper had been read and fairly discussed, while most of the attendants of the meeting were yet in the room, one of our best posted members on the subject approached me and said, he could have easily contradicted a statement made by another member during the discussion, and one contained in the paper itself.

"Why did you not do so?" I asked him. "I am very sorry you did not take the floor at the proper time."

"Well," said he, "I wish some one would have punched me hard at that time, so I would have been compelled to jump from the chair; once up I would have spoken, but I deliberated and hesitated so long, whether I should rise or not, until it was too late." Here you have the whole secret in a nutshell. Now I do not exactly approve the remedy our worthy member suggested, as I am afraid we might need too many "pinches," and they also might at times pinch at the wrong moment. I think, however, there is a simple and more effective remedy.

All that is necessary is to bring our members frequently in contact, so that they become better acquainted with each other socially, that they become friends, then that unwarranted shyness will disappear.

Occasional short excursions, combining pleasure and recreation with scientific purposes, and also occasional entirely informal dinners are desirable means of making our members socially better acquainted with each other. Formal, stiff dinner parties should be avoided. Engineers, as a rule, are averse to empty formalities and have a dislike for swallow-tail coats and white kid gloves, and would much rather stay away from such affairs, than seek them.

The most efficient means, however, to accomplish the desired effect is to have quarters of our own, where members can meet every evening, if they desire, and become acquainted and discuss freely scientific topics. Have our library accessible at any time, and comfortable rooms for it, to peruse our books in such discussions, and not be disturbed by outsiders. To meet once a month in a large uncomfortable hall, where the members, no matter how many there are, look like a flock of lost sheep in a big barn, and where even a familiar face, through its surroundings, appears strange and cold, must have a depressing effect on any one.

Our income being exclusively from our

very moderate annual dues from members, is barely sufficient to defray our most necessary expenses to maintain our organization, and precludes the idea of ever having anything to spare from it, sufficient to be applied toward procuring permanent quarters of our own.

I deem this a proper place and time to appeal to our manufacturers and railroad corporations, with their millions invested in this city and vicinity, with their most important, most vital interests in the hands and under the direct care and management of our members, I am free, I say, to appeal to them, that this would be a proper case for them to take up, and to extend to us their aid for the purpose stated. A moderate sum would be sufficient, and ultimately they themselves would be the largest gainers by the realization of this project.

Trusting that the seed I have sown herewith will fall on fertile soil, and that my successor in office may be fortunate enough to be able to announce in the next annual address to your meeting the consummation of our wishes and the realization of the objects of our appeal, I close with the sincere best wishes for the continued success of our Society and the manifold great interests of our busy city.

Your retiring president,
A. GOTTLUB.

REPORT OF THE TREASURER

For the Year ending January 16, 1883.

1882.	RECEIPTS.		
Jan. 17.	Balance	$187.88	
	Dues from 1 member to January 17, 1882	2.50	
	Dues from 3 members to January 17, 1882, at $5.00	15.00	
	Dues from 160 members to January 16, 1883, at $5.00	800.00	
	Dues from 12 members to January 16, 1883, at $2.50	30.00	
	Dues from 4 members to January 16, 1883, at $5.00	20.00	
	Part payments credited	5.00	
	Unexpended annual dinner fee.	3.00	
	Collected for binding transactions	40.30	
			$1,103.73
	EXPENDITURES.		
	Mercantile Library Association to December 15, 1882	300.00	
	Salary of secretary to October 15, 1882	150.00	
	AMERICAN MANUFACTURER, for printing transactions	48.00	
	James McMillin, for postal notices of meetings	38.70	
	Postage and stationery	28.67	
	E. D. Wilt, badges	15.00	
	W. A. Bunting, rubber stamps	4.50	
	Total		$584.87
	Balance in hands of treasurer		518.86
			$1,103.73

Respectfully submitted,
ALBERT E. FROST,
Treasurer.

Library Fund.

1882.	RECEIPTS.		
Jan. 18.	Balance	$209.83	
	Donation through William Metcalf, Esq	500.00	
	Cash for duplicate copy of Percy's Iron and Steel	25.00	
			$734.83
	EXPENDITURES.		
	Error in balance, previously reported	2.18	
	Books, as per vouchers on file	568.52	
	Binding	112.85	
	Services in arranging for binding	11 00	
			$694.55
	Balance		40 28
			$734.83

Respectfully submitted,
A E. FROST,
Treasurer Eng. Society.

REPORT OF LIBRARY COMMITTEE.

On behalf of the Committee on Library, I beg to submit the following report:

The Committee has added to the library during the year 110 new volumes, making the total number of books owned by the Society 595. The additions made during the year, with a few exceptions, are such only as had been recommended by the several special committees appointed for this purpose last year. The Pittsburgh Library Association were requested to add to their list of periodicals the following:

1. Organ fuer die Fortschritte des Eisenbahnwesens.
2. Allgemeine Bauzeitung, Wien.
3. Deutsche Bauzeitung, Berlin.
4. Zeitschrift fuer Bauwesen, Berlin.
5. Zeitschrift des Architekten und Ingenieurvereins zee, Hanover.
6. Zeitschrift des oestreichischen Ingenieur und Architektenvereins.

The Library Association complied with this request, and these periodicals have been on file most of the year. A later request was made to add also:

The American Journal of Chemistry and Notes, Queries and Answers, Manchester, N. H., and compliance has been promised.

The Annales des Ponts et Chaussees, from the beginning of its publication in 1831 until the close of 1880, which was ordered purchased for the Society last year, was received, and all but the administrative portion of the work has been bound, making 100 volumes of text and 10 volumes of plates. All other books received in paper covers were also bound.

A complete catalogue of the works owned by the Society has been in course of preparation for the past six months, and this work has now so far progressed that it is expected the printed catalogues will be ready for distribution by the next meeting. A list of the scientific periodicals to be found on the tables of the library will be included in the catalogue. It may not be known to all members

that the Society's library is already a very valuable one; the books were selected with great care and comprise all departments of applied science, and the periodicals on file include all important serial publications. The catalogue will, it is believed, greatly enhance the usefulness of this valuable possession. Respectfully submitted,

C. L. Strobel, Librarian.

REPORT OF THE COMMITTEE ON ROOMS.

Pittsburgh, Jan. 16, 1883.

To the Members of the Engineers' Society of Western Pennsylvania:

We respectfully report that—

1. We have found no suitable rooms at a moderate rent.

2. We have had some interviews with parties interested in the Y. M. C. A. building to be put up corner Seventh and Penn avenue; it is very probable that favorable arrangements may be made to give our Society suitable accommodations at reasonable rent in one year from now, as those interested have expressed themselves desirous that our Society should take rooms there.

3. We reccommend that the present arrangement with the Library Association be continued for the current year.

W. Thaw, Jr.,
T. Rodd. } Committee.

REPORT OF SECRETARY.

January 16, 1883.

To the Engineers' Society of Western Pennsyl vania:

Gentlemen:—At the time of my last report there were 221 names on the list of members, to which there have been added 41 names making a total of 262.

During the year past one has died and 23 have resigned, leaving now on the list 239, a net gain of 18.

At the beginning of the first year of the organization of this society we had 110 names on the list of members, the second 190, the third 221, and we now start the fourth year with 239, having more than doubled the membership in three years.

If I am correctly informed your society is the largest local society in the United States, and has also a better average attendance than any other. During the year the society has held nine regular meetings, as follows:

	Membership.	Attendance.
Jan. 17, 1882, annual meeting	221	46
February 21, 1882	236	32
March 21, 1882	238	41
April 18, 1882	242	48
May 23, 1882	247	44
September 19, 1882	254	30
October 17, 1882	256	36
November 21, 1882	262	61
December 19, 1882	262	44
Total		382
Average	246	42

The average attendance is less than for the last report, being about 17 per cent of the membership.

The following papers have been read:

February 21.—The Absorption of Metallic Oxides by Plants; by Prof. F. C. Phillips.

March 21.—The Slide Rule; by Edwin Thacher.

April 18.—Combination Bridges; by C. L. Strobel.

May 23.—Pittsburgh Sewerage; by A. Dempster.

Sept. 19.—Furnaces and Combustion; by Ign. Hahn.

Oct. 17.—Petroleum; by Max Livingstone.

Nov. 31.—Actual Strength of Bridges; by Fred. Melber.

Dec. 19.—Inspection of Bridges; by W. S. Thompson.

All of these have been published except those of May 23 and Nov. 21, the author of the first being too busy to revise the stenographer's report.

The discussion of a paper after being read by the author has been more general the past year than in previous years, and this is encouraging. This is due probably to the better acquaintance we have with one another, which is one of the main objects of the organization.

At the April meeting the society requested all the members that might attend the Annual Convention of the Am. Soc. of C. E. at Washington, to use their influence towards the continuance of the Board for the Testing of Iron and Steel. Several of our members attended said meeting, and so far as possible tried to carry out the wishes of the society. Thus far, however, Congress has taken no decided action. It is presumed your Committee on Tests will report more fully on this subject.

The condition of your finances will be given by your treasurer. But it may be well to bring to your notice the necessity of promptly paying all dues. The publications of the society can be issued in better form and additions made to the Library if members will attend to this matter.

During the year just closed 60 members have failed to remember the treasurer.

In case any member desires to sever his connection with the society, it would be well to first pay all back dues, and then notify the secretary of his desire to resign.

It is presumed that in most cases the failure to settle dues is on account of negligence. I would suggest the propriety of requiring all applicants for membership to deposit with the application the dues of the first year, which could be returned to him if not elected to membership. During the past year the society attempted an annual dinner in February and an excursion in June, both of

which I regret to say, were failures in point of members attending.

The officers of your society may receive encouragement from you, and perfect all arrangements, but if you do not attend as individuals the object will be a failure.

The board of direction has held eleven meetings during the year, and has tried to so direct the affairs of the society as to make it a success.

The question of room has been one of considerable interest, and various plans to better our condition have been discussed, but no final conclusion has been reached.

The AMERICAN MANUFACTURER still continues to publish the papers monthly. During the past year the board decided to republish all the papers in a better form, previous to July, and the republication is now being distributed to you.

The society, in exchange for its publications, has received the following papers:

Annals de la Sociedad Cientifica, Argentina, Buenos Aires.

Ingeniors Foreningens Forhandlinger, Stockholm.

Revista de Obras Publicas E Minas, Lisbon, Portugal.

Die Anlage Betrieb der Eisenhutten, Liepsig.

Transactions of the Am. Soc. Civ. Eng., New York.

Transactions of the American Institute of Mining Engineers.

American Engineer, Chicago.

Proceedings of the Engineers' Club of Philadelphia.

Journal of the Society of Arts, London.

Journal of the Association of Engineers' Societies, New York.

Report of the Institution of Civil Engineers, London, S. W., and others.

I regret to close with the announcement of the death of one of our young members, W. S. Dwight, who died Jan. 3, 1883.

Respectfully,

JAMES H. HARLOW, Secretary.

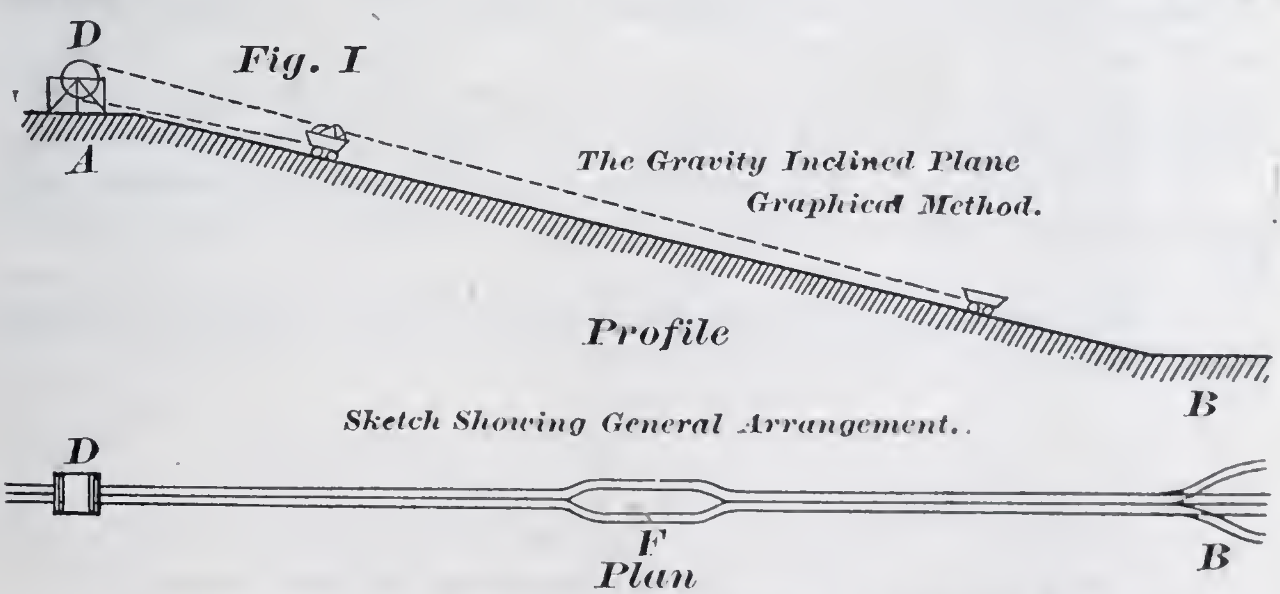
THE GRAVITY INCLINED PLANE.

An Investigation by the Graphical Method.

BY S. B. FISHER, C. E.

[A paper read before the Engineers' Society of Western Pennsylvania, February, 1883.]

The motive power in the gravity inclined plane, is the force of gravity. These planes are used in transporting material from a higher to a lower level. The valley of the Monongahela is thick with them, where they are used to convey coal from the mouth of the pit to the barge. They are also extensively used in the limestone region of the Mahoning and Shenango valleys for loading cars with limestone. The general arrangement of the plane as used for the latter purpose, is shown on Fig. I, where *A B*, in plan and profile shows the plane. On the plane is laid three rails throughout its entire length. At the middle *F*, called "the parting," the center rail splits and diverges, so to speak, so as to form a section of four rails for the passing of trains. These trains are composed of two or more small cars



coupled together. They are raised and lowered by means of a wire cable. This cable successively coils and uncoils itself on the drum *D*, at the head of the plane. The axis of this drum is generally horizontal. One cable is led from the upper side, and the other from the under side of drum, the

so that when the drum revolves, it will cause one cable to wind up and the other to unwind, at the same time. The power to turn the drum is transmitted through the uncoiling cable from the descending train. The power transmitted through this cable must be at least sufficient to overcome all resistances of parts and pull up the empty train, and to do so at a certain practical or reasonable speed.

The forces in this problem, divide themselves naturally into three groups.

1st. Forces due to constant weights—cars and cable.

2d. Forces due to the variable load—material lowered.

3d. Forces due to friction, resistance of air, rigidity of cable, and all other retarding influences.

The first two groups of forces, can be determined when we have the weights of cars, cable, plant and load, and the data—namely, length and inclination of plane.

We will proceed at once with the investigation of a specific plane. The plane chosen is one in the Mahoning valley, operated by the Lawrence Lime Company, built within the last year, laid out by Mr. Ellis Morrison, of New Castle. We are indebted to the courtesy of Mr. Morrison, for the data of this plane.

	lbs.
We have for each cable length, 2360 feet @ 1.58 lbs. per foot	3730
For each empty car	1700
For each net load	7000

On this plane four cars are run coupled together in each train. We have therefore:

Group 1. The cable plus eight empty cars, 3730 + 8 × 1700 = 17330 lbs.

Group 2. Four net loads, 4 × 7000 = 28000 lbs.

The third group of forces when we tackle the problem, is unknown. Further on, some observations will be given from which we will determine the total resistance. We will not enter into an analysis of the various elements of this resistance, the resistance of air, friction of drum, rolling of car wheels, rigidity of cable, etc., but try to get the total retarding force.

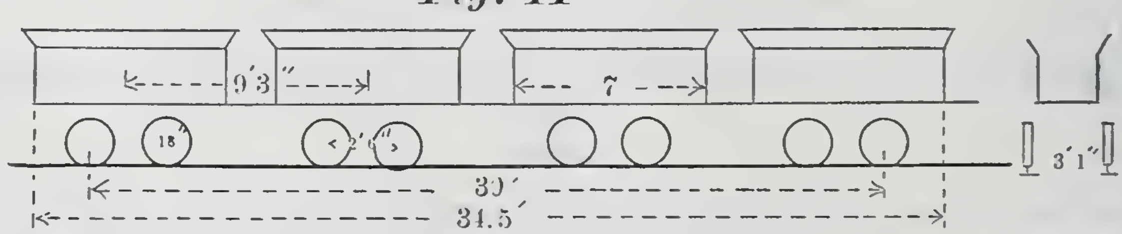
For convenience in investigation, we will arrange the known forces into three groups, as follows:

1. *The gross load.*—Cable, plus eight cars, plus four net loads, plus the moving plant, 4750 + (8 × 1700) + (4 × 7000) + 10450 = 56800 lbs.

2. *The down load.*—Four cars, plus four net loads, plus nothing varying to weight of cable, (4 × 1700) + (4 × 7000) + (0 ∝ 3730) = 34800 lbs. ∝ 38530 lbs.

3. *The up load.*—Four cars, plus weight of cable varying to nothing, (4 × 1700) + (3730 ∝ 0) = 10530 ∝ 6800 lbs.

Fig. II



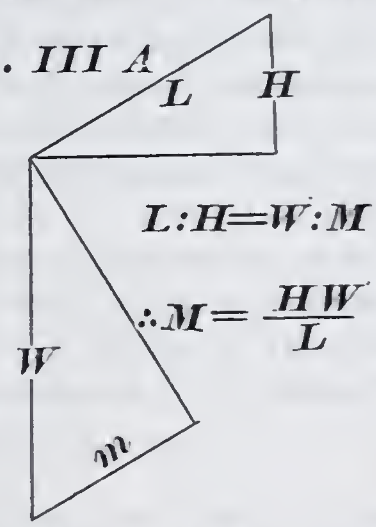
The two latter groups of forces, the down load and the up load, will actually be loads distributed over the points of wheel contact. See Fig.

II. The wheel base is 30 feet. We will first regard all these forces as concentrated at the centers of gravity of the up and down trains respectively, and afterwards indicate the effect of the distributed load.

In Fig. III, *A B D E F G H K N O P* shows the various sections of the plane under consideration.

As the down load starts off, its center of gravity will pass through the center of the train. As the train descends, the uncoiling of the cable will cause this center of gravity to shift upward from the center of the train, with a rate of motion directly proportional to the speed of descent; at the foot of the plane, this center of gravity with a cable length of 2360 feet, will lie $\frac{(1180 + 15) \, 3730}{38530} = 116$ feet above the middle point of the train. The locus of the *mean* center of gravity, therefore, of the down load, lies between the middle of the train and a point 116 feet up the plane from it. We say position of the *mean* center of gravity, because the flopping up and down of the cable will cause the center of gravity to vibrate from its stati-

Fig. III A



cal position—up and down the plane—to a degree, dependent in measure on the amplitude of the oscillation of the cable.

In like manner the locus of the center of gravity of the up load, lies between $\frac{1195 \times 3730}{10530} = 423$ feet up the plane from the train center, and the middle point of the train.

The motion of the down train will be used as the basis in this investigation. The motion of the up train, will be almost an exact counter-part of that of the down train, on end for end of the plane.

Now the observations to determine the effect of free resistances, must be taken before the application of the brake. This in the case under consideration will be on the upper half of the plane. The position of the center of gravity of the down load at the end of this upper half will be $\frac{605 \times 1865}{38530} = 29$ feet above the middle of the train—or within a distance equal to the wheel base, from its true position. As our observations to determine the resistances, were taken from the middle of the train, we will be compelled to use this point, as the point of the application of the resultant forces. Nor will this practically change or vitiate our results; because first, it is the actual motion of the down train we seek, rather than that of a theoretical center of gravity; second, the motive force due to the cable, will always partly, and sometimes entirely be applied in overcoming

the friction of rollers at or near the points where it is developed; and third, whatever variation this may produce, it will appear in the gross total of resistances.

Having staked off the ground, we are now ready to dig into the problem.

Let $A\ B\ D\ E\ F\ G\ H\ K\ N\ O\ P$ be the plane, plotted without distortion of the vertical scale, *i. e.*, vertical scale equals horizontal scale 200 feet to 1 inch. The sections with the length and rise of each, are shown in the first three columns in the tabular statement.

Let us consider first, the down load, excluding the cable. Take any point B, on the first section $A\ B$, and regard the center of gravity of the down train as passing through it. On the vertical through this center of gravity, we lay off the total weight of the down train 34800 lbs. to a scale of 20,000 lbs. to one inch. This weight is represented by BR, in the diagram. This weight is decomposed into two components; one of them perpendicular to, and the other parallel to the section of the plane. The former is supported by the track; the latter is the motive force. From the extremities of BR, drawing lines respectively parallel and perpendicular to the section of the plane, and prolonging them to their intersection L, we get lines which represent these components, scaling them with the same scale to which BR is laid down, we get their values. In this case, we only want the value of the component parallel to the plane, or the motive force. We can also get this motive force from the similarity of triangles as follows. See Fig. III A, where L = length of plane, H = rise or height, W = known weight and m = unknown motive force.

$$L : H = W : m = \frac{H\ W}{L}$$

The components of the down train, for each section of the plane, are thus determined and tabulated in column headed "Motive Weights Down."

To show these motive forces graphically, we lay off to scale from a convenient horizontal A P, the components $R\ L$, each in its proper section. As these components are small comparatively, they are laid off twice in the diagram, thus doubling the scale. We thus have an area $A'''P'''P'$ $O'\ldots A'A'''$ at any point of which by scaling vertically we get the motive force due to the down train.

Fig. III B

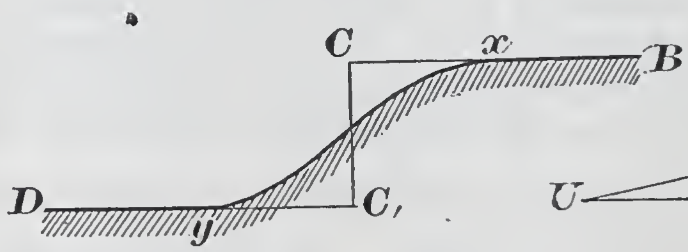
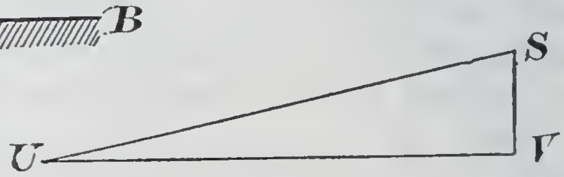


Fig. III C



We consider next, the action of the up train. The retarding force of it is obtained in a way precisely similar to that by which the motive force of the down train was obtained, by laying of BR = 6800 lbs. and scaling $R\ L'$. We can also get it, by using the above formula, $m = \frac{H\ W}{L}$.

These results are tabulated in the column marked "Motive Weights Up."

The conditions of the problem are such, that the effect of the up load at the lower end of the plane is felt on the down load at the upper end of the plane; at the passing points, the effects of the loads act directly opposed to one another; while on the lower part, the effect of the down load is opposed by the effect of the up load on the upper part. If now we imagine the track on which the up load moves, to be revolved around its middle point, until it coincides with the track on which the down load moves, marking where the points $C\ L\ M$, etc., come, we will subdivide the plane into sections, symmetrical with respect to the middle point of the plane and simultaneous and homogeneous, so to speak, with reference to the effect of the up and down loads. These sections are shown on the diagram, and are the sections of the tabular statement.

We will now look at the cable load. At the starting point of the down train, the weight of the cable is zero, at the middle point, it is one-half its total weight and at the foot it is the total weight of cable. We will take the direct line $A\ P$, to represent the inclination of the cable, as it would be a useless practical refinement to decompose the cable on each section. The component parallel to the plane is represented by $R^{II}\ L^{II}$, making $P^{III}\ P^{IV} = R\ L^{II}$ and drawing $P^{IV}A^{III}$ we have a triangle $A^{III}P^{III}P^{IV}$ as a positive addition to our load area. A similar triangle $P^{IV}A^{IV}A^{III}$ will evidently represent the negative addition of the cable. See column 6 for the net effect of the cable.

The effect of the distributed load on the load area is shown in enlarged scale in Fig. III B, where $D\ C^1$ and CE, are limiting lines of the net motive forces as shown on main diagram. Laying off $Cx = C^1y = 15$ feet, one-half the wheel base, and drawing the reversed curve xy through the middle point of CC^1, we show graphically the rounding off of the load area corners produced by the distributed load. If the different sections of the plane, are connected by vertical curves in the track, as they should be, this rounding off will be increased to a corresponding degree. Making these constructions in the main diagram we have the graphical representation of the motive forces complete. It is shown shaded. At any point of this area, by scaling vertically, we get the net motive force. These net motive forces, taken in the middle of the section on account of the variation of the ends produced by the cable, are shown in the table in column seven.

The next thing we look at, is the effect of these motive weights, or accelerating forces on the gross load. We lay off the gross load $A\ S$ to the same scale as used for the motive forces. Horizontally from this we lay off $ST = g = 32.16$, the acceleration produced by gravity, and connect $A\,T$.

Now at the points where the net motive forces are scaled, at the middle of every section we construct triangles similar to AST and scale off and tabulate the corresponding accelerations g. See table column eight.

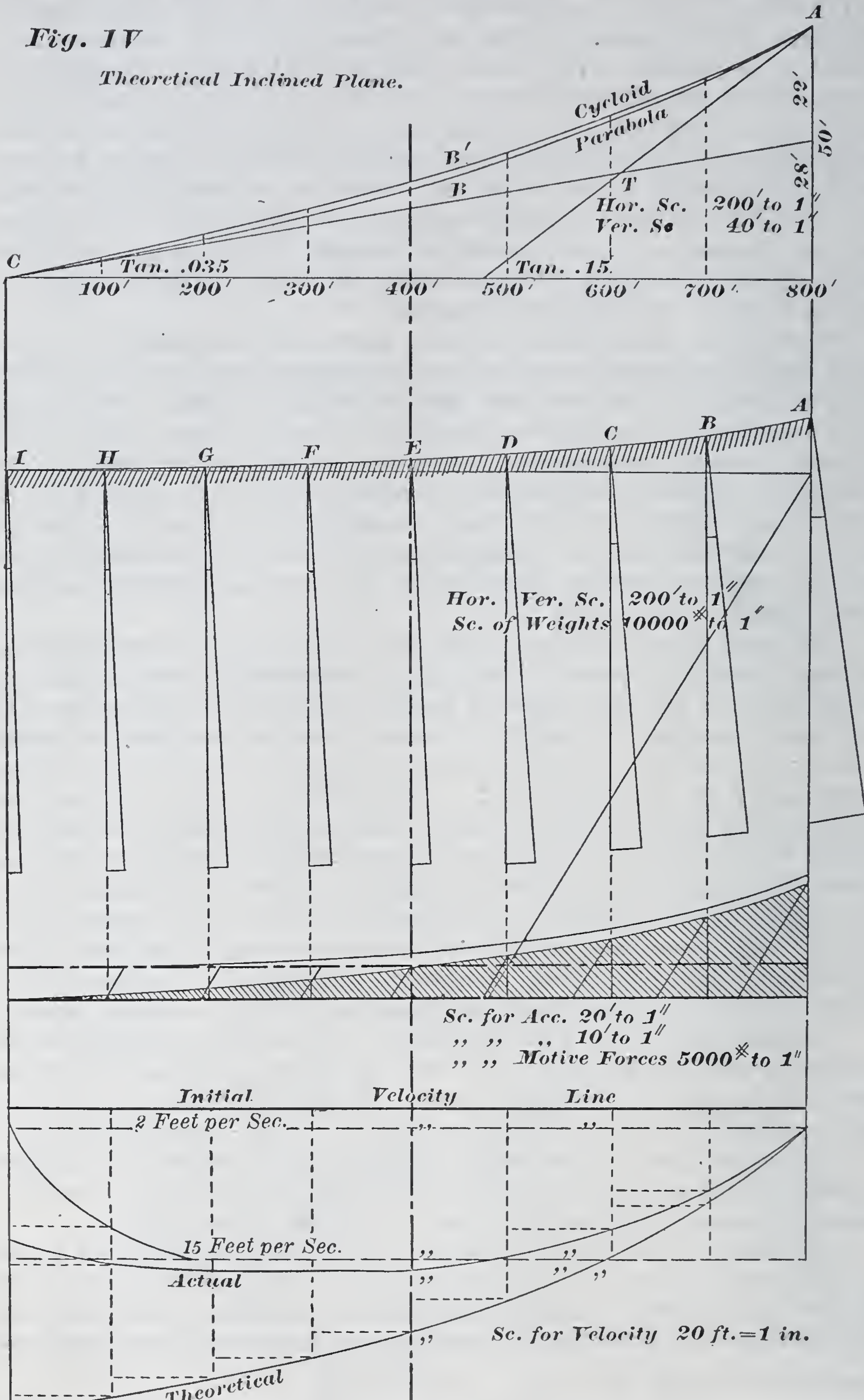
The action of the motive force here, is precisely similar to the action of the small bar, used in Atwood's machine, as we remember in our school days, to experimentally demonstrate the laws of gravity. If $m =$ the motive force; $M =$ the gross load, $g =$ gravity and g^1 the acceleration—then from our similar triangles $M : m = g : g^1$ or $g' = \frac{mg}{M}$.

Velocity.—The next thing to look at, is the velocity which these accelerations would produce on their respective planes if allowed to act without

resistance. We apply here the principle of physics, illustrated in Fig. III.

Fig. IV

Theoretical Inclined Plane.



C. Let $S\,U$, be any inclined plane. If a body moves down this plane, $S\,U$, its velocity measured in the direction of the plane, will be the same as its

vertical velocity would be at the point V, in the same horizontal with the point U, the initial velocity at the point S, in both cases being equal. See Gregory's Hutton's Mathematics in the chapter on motion on inclined planes. The ordinary formula for finding the velocity of a falling body $v = \sqrt{2gh}$, can thus be directly applied to get the velocity in the direction of the plane, generated on each section by substituting the height h, and acceleration g, proper for each section. A graphical solution of this quadratic equation is shown in Fig. III. D. Describing on the height H, a semi-circle, and laying off from one end, $2g$, we have the velocity v, represented by the chord of the arc, of which $2g$ is the versed sine, to diameter H. For from similar triangles, $h : v = v : 2g \therefore v = \sqrt{2gh}$.

These velocity increments are tabulated in column nine. The initial, final and mean velocities for each section are shown in columns ten, eleven and twelve. The quantities in columns ten and eleven are obtained from nine by cumulation.

It is supposed we pass from plane to plane, without loss of velocity from impact. This will be the case practically on account of the distributed load, and on account of the adjustment curves connecting successive sections. If there is any loss from impact, it will be included with the other resistances. These velocities are plotted on the diagram and form the theoretical velocity curve.

Time.—The times of descent on each plane, are obtained by dividing the length of plane by the mean velocity. See column thirteen. The total times in passing through successive planes are shown in column fourteen, and are obtained from column thirteen by cumulation. These last values are plotted on the diagram and constitute the theoretical time curve.

In column fifteen, are given the actual observed times of descent. These observations were taken a few weeks ago by Mr. Morrison and the writer, with trains of three cars and repeated subsequently with trains of four cars, under slightly different conditions. The results from both series of observations are substantially the same. The plane was divided into the sections shown on the diagram, and the time of passing the successive points noted. The mean of several observations are given. These times are cumulated in column sixteen. In column seventeen, are given the actual mean velocities, obtained by dividing the length of plane by the actual time of descent. The values in columns sixteen and seventeen are plotted and form the actual velocity and time curves.

We have now, all the elements of the problem before us ready to be read off. Let us examine the time curves. There are three marked periods in the time of descent.

1st. The starting period.

2d. The period of the action of free resistances.

3d. The period of the action of the brake.

We will call the passage over the sections A-B and B-C the starting period. The actual time occupied by this descent over the section A B is 9 seconds. The time occupied by the up train, going over the equal section P O is 14 seconds. This variation in time, seems to be caused mainly by taking up the slack in the cable. The slack in this plane is somewhat excessive, as during repose it will lie on the concave surface A B C E F G. This fact has an important bearing on the velocity acquired during the

starting period. If this velocity was uniformly accelerated on the section $A\ B$, starting from zero and occupying 9 minutes to descend, the average velocity would be $\frac{88}{9} = 9.8$ feet per second and the final velocity 19.6 feet per second. At any point, see Fig. III. E, we get the velocity by scaling vertically from $A\ B$, to the 9 seconds line. If on the other hand we take the 14 second line as the time of descent, we have a final velocity of $\frac{88}{14} \times 2 = 12.6$ feet per second. We actually have neither of these velocities but something like that shown by the curved line $A'\ B'$, where AA' represents a certain initial velocity on entering the plane, produced by a starting shove, or the inclination of the approach. As the train enters the first section, at first there is no resistance of the up load at all. The slack of the cable being taken up little by little, produces increasing resistance, thus retarding the acceleration, until the upload is under way. After this, the resistance begins to diminish, on account of the relative positions of the up and down cables. Scaling to a line similar to $A'\ B'$, we get the actual velocities. These relations are shown very plainly in both velocity and time curves on the diagram. During the starting period, the actual time curve has less ordinates, and the actual velocity curve greater ordinates, than the theoretical curves, the theoretical and actual curves crossing each other. The profile to which this upper section of the plane was first constructed is shown by the dotted lines $A'\ B\ C$ in Fig. III. This path did not give sufficient velocity in passing through some of the subsequent upper sections to do business with dispatch, especially when the weather was unfavorable. So the approach was carried out a distance of some 90 feet, beyond the original head of the plane.

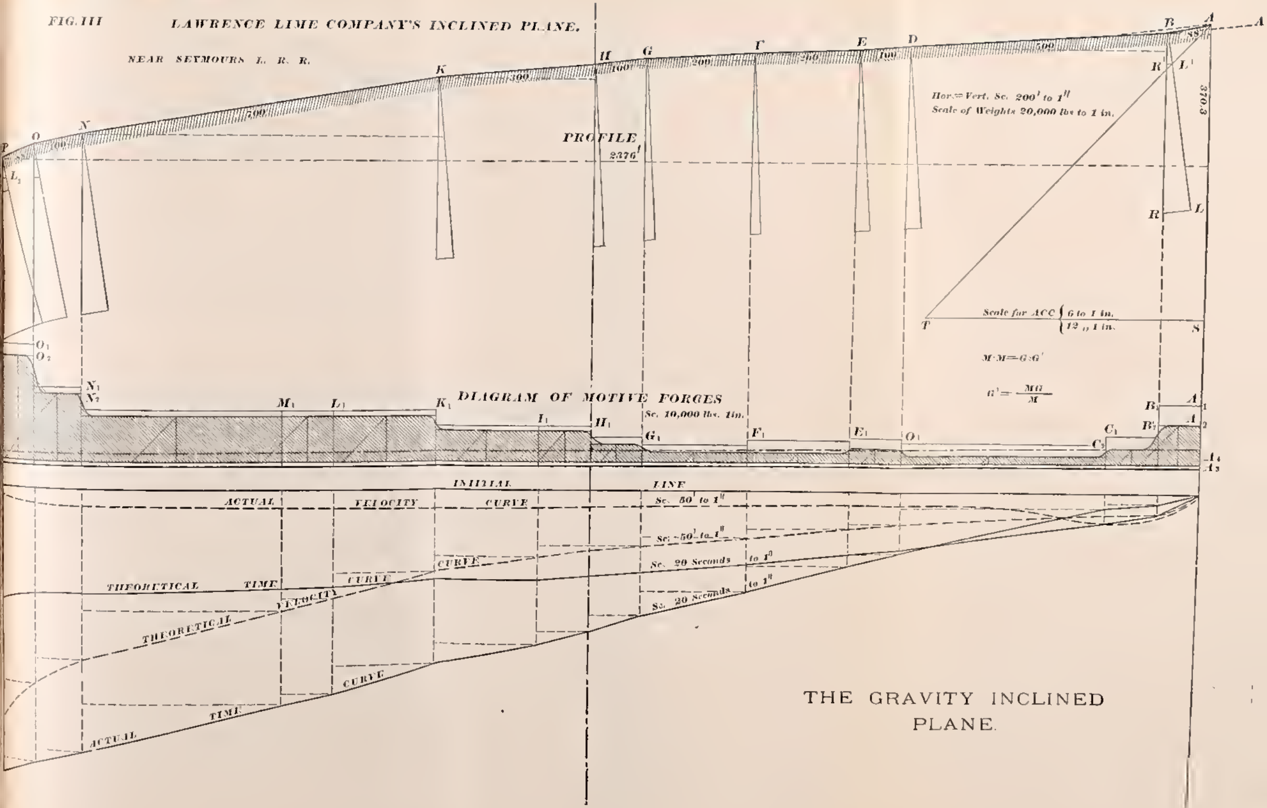
This plane was first operated with two cars coupled. When they were stopped for any cause on some portions of the plane, they would not start themselves, but had to be urged by a pinch bar. The train was increased to three cars, when it worked better. It is now operated with four cars coupled, using heavier cable. Our observations were taken with a train composed of four cars.

The final velocity of the starting period, is thus about 14.5 feet per second. This is the initial velocity of the second stage—the period of free resistances. This period extends over the next four sections, to the middle of the plane. We determine the resistance on the basis of the squares of the velocity which are represented by the ordinates of the velocity curves being proportional to the accelerating forces. We can also get the same result from the squares of the times, represented by the sectional increment of the time curves, being inversely proportional to the acceleration. These forces, we find to average on these four sections 1092 ℔s. acting parallel to the plane. This retarding force varies on different sections from 1047 ℔s to 1184 ℔s—some 12 per cent. of variation. The principal cause of this variation, seems to be from the friction of the cable, which often slides on rollers which will not turn. Taking the average resistance at 1092 ℔s we have the following relations:

1092 $<$ 2 per cent. of gross moving weight, 56800 ℔s. = 40 ℔s. per ton of 2000 ℔s.

1092 $<$ $2\frac{1}{2}$ per cent. weight of trains and cable 45330 ℔s. = 50 ℔s. per ton of 2000 ℔s.

1092 $<$ 4 per cent. of weight of net load, 28000 = 80 ℔s. per ton of 2000 ℔s.



THE GRAVITY INCLINED PLANE.

starting peri
A B, starting
velocity wou
second. At a
cally from A
ond line as t
per second.
like that sh
initial veloci
inclination c
there is no
taken up litt
acceleration
gins to dimi
cables. Sca
These relati
the diagram
ordinates, a
retical curve
profile to w
shown by th
cient veloci
to do busin
So the app
original hea

This pl
stopped for
themselves,
to three ca
coupled, us
composed c

The fin
second. T
resistances.
dle of the p
of the vel
curves bei
same result
crement of
tion. The
acting par
tions from
pal cause c
often slide
at 1092 lbs

1092 <
2000 lbs.

1092 <
ton of 2000

1092 <

The third period, as seen at a glance, from the changing ratio of the ordinates of the curves, begins at the section *GH*. Part of the increased resistance shown on this section, however, is caused by the short and sharp reversion in turning out by both tracks for the parting. The value of the investigation in this third period consists in the hint which we get of the power required for the brake, and in the determination of the cable stress. We can see this more plainly from the load area, by plotting the resistance

FIG. III D

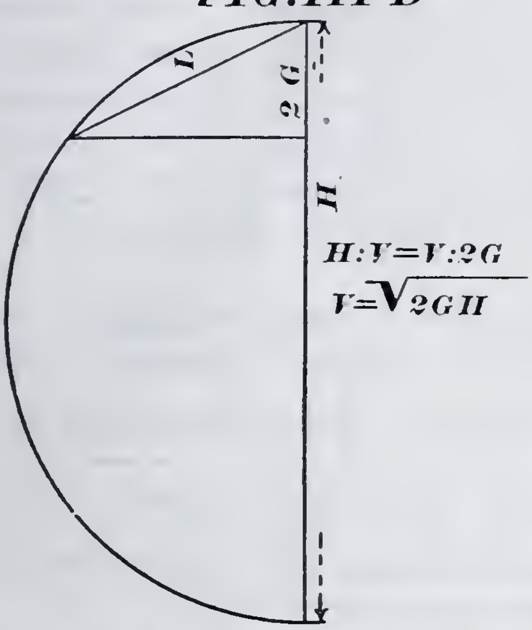
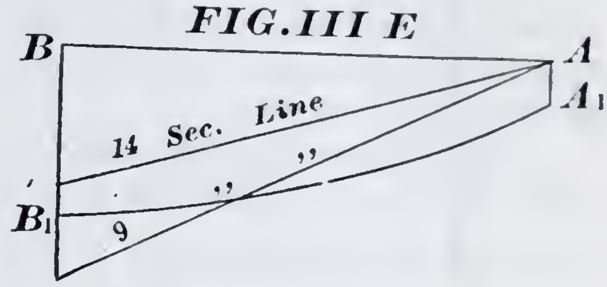


FIG. III E



as a negative load. In the section *PO*, we have still a force of 9610 lbs. to be sustained by the cable and absorbed by the brake. The brake should have sufficient power beyond this to take up the momentum and stop the train on any part of the plane. If the up train should become disconnected from any cause, the brake should be still able to hold the total descending train. This on the section under consideration will increase its power some 1600 lbs. The power of the brake should therefore be at least 11,200 lbs. The maximum stress to which the cable is subject, is the gross down load's motive force, less the retarding influence of friction at the train, (which we may take at 1 per cent. of the down load) or 10,000 lbs. A grade of 1 foot per 100 in the case we are considering will develop a statical force, parallel to the plane of 280 lbs. Therefore, a plane with an inclination of 3.9 feet per 100, will just balance the gross resistance. This, therefore, will be the limiting minimum inclination of plane, to traverse with a previously acquired speed. If to this we add, the very desirable condition, that the trains must start themselves when stopped at any point, we will have to increase this minimum grade.

With these points, viz: the gross resistance and the minimum grade and with the data of plane, our path to the solution of the resulting forces, in any given case, will be considerably shortened over the one we have just been traversing.

We will now devote a few minutes to the consideration of the theoretically correct profile for an inclined plane. The profile we seek, is the one which will cause descent in the shortest possible time—or in mathematical language we want the brachystochrone. We have two conditions given, first, the motive forces are parallel, and second, the bodies move in the vertical plane of these forces. We know the general form of this curve of

quickest descent for a single body moving in a plane to be an arc of the common cycloid. This problem has attracted much attention in the mathematical world, and was solved two centuries ago, by both Newton and Bernouli. It is treated very concisely in Pierce's Analytic Mechanics—Article 572, in the chapter on the Brachystochrone.

This curve is shown in Fig. IV. top, $AB'C$, the vertical scale being exaggerated to five times that of the horizontal. The plane here shown has a base of 800 feet, and a height of 50 feet. An inclination of 2° corresponding to a grade of 3.5 feet per 100, is given the lower tangent. This limiting grade is a tangent CT, to the vertex of the cycloid, and the cycloid ABC is simply passed through the summit of the plane. The equations for this cycloid will be given in appendix A. A parabola ABC, is also shown, defined by same tangent CT, and an upper tangent AT. Both equations and a graphical method for constructing this parabola will be given in appendix B.

Now instead of passing the cycloid tangent to CT and through A, we pass it tangent to both CT and AT, using the intrinsic equation, with point A as origin, $Z = a \sin^2 \frac{Z}{S}$. In this equation Z is the intercept of the tangent to the curve on AT, a is a constant and $\frac{Z}{S}$ is the arc or angle which the tangent makes with AT. This coincides almost with our parabola. For all practical purposes the two curves are interchangeable.

The prominent idea in the practical use of this concave profile being to give the train its limited maximum velocity as soon as possible, which it should preserve as long as possible, to leave the plane with a very low velocity. On the other hand, a body descending theoretically the curve of quickest descent, receives continued increments of velocity and leaves the plane with an unlimited maximum velocity.

Taking two cars coupled together as making a train to run on a plane having the parabolic profile ABC, we have:

	Lbs.
Gross load	30000
Down train	20000
Up train	5000 lbs. and cable 1000

The motive force of the down load is found at various points, by taking the component in the direction of the tangents to the parabola at the points. The retarding forces are treated in the same way. The upper boundary lines of these areas are symmetrically curved being thus in marked contrast with the actual plane shown in Fig III. The cable is shown as in Fig. III, and finally the resistance taken at 2 per cent, or 40 lbs. per ton of the gross load = 600 lbs. From these net motive forces the acceleration is found as before at intervals of 100 feet. From these accelerations the velocities are computed. The initial and final velocities are taken at 2 feet per second. As soon as the train attains its maximum velocity of 15 feet per second, the brake must be applied. This effect of the brake is shown by the 15 second and actual velocity curves, the portions of the ordinates included between these two curves indicating the absorption of forces by it. The maximum cable stress is shown by the greatest down load motive force ordinate, less that portion of the retarding force due to resistances at the cars—say 10 lbs. per ton or ½ of 1 per cent of the down train weight.

TABULAR STATEMENT.

SECTION.	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18
				Motive Weig'ts. $\left(\frac{H.W}{L}\right)$				THEORETICAL.							OBSERVED.			
										Velocities.			Time.		Time.			
	Length.	Height.	Grade.	Down.	Up.	Cable.	Resultant.	$\frac{m.g}{M}$ Acceleration.	$\sqrt{2gh}$ Velocity Increments.	Initial.	Final.	Mean.	Partial.	Total.	Partial.	Total.	Velocity.	Resist-ance.
				+	—	—	+						Seconds.					
$A—B$	88	15.92	18.10	6270	2260	406	3604	2.030	8.037	0	8.036	4.018	21.90	27.90	10	10	8.8	
$B—C$	100	8.55	8.55	2975	1278	372	1325	.750	3.620	8.036	11.656	9.844	10.16	32.06	7	17	14.3	
$C—D$	400	27.51	6.88	2394	1221	283	790	.447	4.961	11.656	16.617	14.136	28.30	60.36	41	58	9.3	1075
$D—E$	100	6 18	6.18	2151	946	195	1010	.572	2.659	16.617	19.276	14.946	5.57	65.93	15	73	6.7	1047
$E—F$	200	12.20	6.10	2122	801	142	1179	.667	4.036	19.276	23.312	21.259	9.39	75.32	27	100	7.5	1047
$F—G$	200	11.03	5.51	1983	653	71	1259	.713	3.966	23.312	27.278	25.295	7.90	83.22	25	125	8.0	1183
$G—H$	100	5.83	5.83	2026	443	18	1565	.886	3.214	27.278	30.492	28.865	3.46	86.68	14	139	7.8	1490
						+												
$H—I$	100	6.51	6.51	2265	396	18	1885	1.067	3.728	30.492	34.220	32.356	3.09	89.77	12	151	8.5	
$I—K$	200	19.21	9.60	3343	375	71	3039	1.721	8.130	34.220	42.351	38.285	5.23	95.00	21	172	9.5	
$K—L$	200	23.57	11.62	4101	415	142	3828	2.167	10.100	42.351	52.459	47.405	4.22	99.22	24	196	8.3	
$L—M$	100	13.91	13.91	4841	420	195	4616	2.614	8.527	52.459	60.986	56.722	1.76	100.98	12	208	8.5	
$M—N$	400	71.85	17.95	6251	468	283	6086	3.446	22.252	60.986	83.238	72.122	4.80	105.78	48	256	8.3	
$N—O$	100	18.80	18.80	6542	581	372	6333	3.586	11.621	83.238	94.859	89.048	1.12	106.90	13	269	7.8	
$O—P$	88	29.23	33.22	11560	1230	406	10706	6.062	18.824	94.859	113.683	104.271	.84	107.74	13	282	6.8	
	2376	270.30											107.74		282			

Av. resistance 1092 lbs.

1092 < 2 per cent. of 5680 (gross moving weight) = 40 lbs. per ton of 2000 lbs.
1092 < 2½ per cent. 45330 (weight of train and cable) = 50 lbs. per ton of 2000 lbs.
1092 < 4 per cent. of 28000 (weight of net load) = 80 lbs. per ton of 2000 lbs.

Referring to the motive force areas, we see that, that of the down load

has its maximum at the upper end, while at the same time that of the up load is a minimum at the lower end of the plane. Both these conditions favor the speedy acquisition of the maximum velocity, while at the same time the total effect of the cable is thrown in the opposite direction. The diagram shows these influences in their relative proportions. The inclination of the upper tangent in the diagram is such as to make the retarding force of the up load at the upper end of the plane, equal to the acceleration of the down load at the lower end of the plane—in other words, to make the motive forces on leaving the plane, equal to zero.

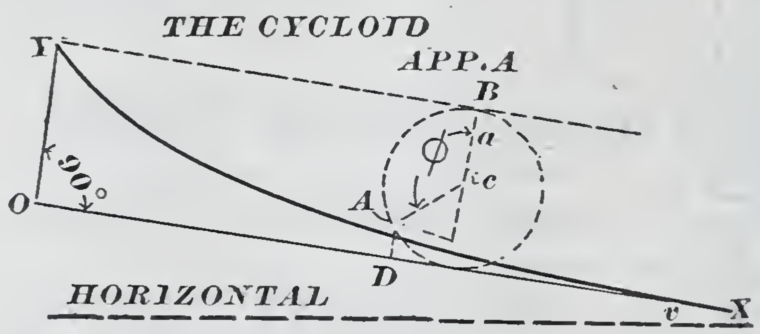
A word as to limiting grades and we will quit. If we suppose the point A to rise, and increase the height of the plane, while the base remains constant, the tangent $A\,T$ remaining parallel to itself, the line of quickest descent will loose more and more of its curvature, until when the tangent $A\,T$ reaches the point C, it will become a straight line. The grade at which the curve of quickest descent becomes a straight line, for the class of planes we are considering appears not to exceed some 10 feet per hundred. When the grade exceeds this limit, the more resistance to the deacending train the better it is, for it tends to diminish the cable stress. On the other hand, if the point A descends, lessening the height, all parts should be carefully and accurately constructed, with a view to diminishing the resistance; the lower tangent may approach, and in extreme cases even attain its limiting horizontal position, and the profile should be accurately constructed to a parabolic or cycloidal arc.

APPENDIX A—THE CYCLOID.

Let the curve be referred to the rectangular axes $O\ Y$ and $O\ X$, $O\ X$ being tangent to the vertex V.

Let a represent the radius of the generating circle.

Let ϕ represent the angle or the arc $A\ B$, which the radius of the generating point makes with the axis of Y.



Then if x and y are the cartesian co-ordinates of the cycloid referred to the origin O, we have the following equations:

(1) $y = a + a\ cos.\ \phi = a\ (1 + cos.\ \phi)$.

(2) $x = a\ \phi - a\ sin.\ \phi = a\ (\phi - sin.\ \phi)$.

Example.—To pass a cycloidal arc through the points V and A on the figure, where $V\,D = 800$ and $A\ D = 50$.

First we find a, by trial, thus:

(3) from (1) $50 = a + a\ cos.\ \phi\ \therefore\ cos.\ \phi = -\dfrac{a - 50}{a}$.

(4) from (2) $a\,\pi - 800 = a\ (\phi - sin.\ \phi)$.

Assuming values for a, we find the value of $cos.\ \phi$, which gives corresponding values of ϕ and $sin.\ \phi$ for (4).

(4) may be written $(a\,\pi - 800) - a\,(\phi - sin.\ \phi = O$, which will only be true for the correct value of a.

For any assumed value of a, whether correct or not we have:

$$(5)\ (a\,\pi - 800) - a\,(\varphi - sin.\ \phi) = \pm\ \Delta.$$

If a = 3400,	Δ = 440.4		If a = 1608.8	Δ =	.06
" 3200,	" = 353.8		" 1608.7	" =	.01
" 3000,	" = 295.6		" 1608.5	" =	—.19
" 2200,	" = 225.6		" 1607.5	" =	—.22
" 1800,	" = 46.4		" 1600	" =	—1.32
" 1610,	" = .42		" 1500	" =	—12.2
" 1609,	" = .09		" 1000	" =	—169.5

For the correct value of a; $\Delta = o$.

We can thus by a series of approximations, make Δ to differ from zero by less than assigned difference. These approximations are carried in the example to the nearest hundredth of a foot, which gives a, correct to the nearest tenth. Usually, it will be sufficient to get a to the nearest foot. In our present example $a = 1608.7$. Substituting this value in (1) and (2) we get:

(6) $y = 1608.7\ (1 + cos.\ \phi)$.

(7) $x = 1608.7\ (\phi - sin.\ \phi)$.

If we make ϕ the independent variable we have

$\phi = 180°$	$\pi\,a - x = o$	$y = o$	$\phi = 170°$	$\pi\,a - x = 560.2$	$y = 24.436$
" = 178	" 111.4	" = .967	" = 168	" 668.2	" 35.150
" = 176	" 224.5	" = 3.92	" = 166	" 782.3	" 47.79
" = 174	" 336.7	" = 8.82	" = 165° 40′ 40″	" 800.	" 50.
" = 172	" 448.5	" = 15.617			

If we wish to make x the independent variable

(7) becomes (8) $(\phi - sin.\ \phi) = \dfrac{x}{a} = \dfrac{x}{1608.7}$.

To use which it will be necessary to construct a table for $(\phi - sin.\ \varphi)$, or the values of an arc minus its sign. Having in a parallel column of the table the values of $cos.\,\phi$ or $(1 + cos.\ \phi)$, we substitute the latter values in (6) and get simultaneous values of y.

Let τ represent the angle formed by any tangent of the cycloid with the axis of X, then by differentiation of equations (1) and (2) and division of same, we obtain

$$tan.\ \tau = \frac{\delta\ y}{\delta x} = -\frac{sin.\ \phi}{1 - cos.\ \phi} = -\ tan.\ (90 - \frac{\phi}{2}).$$

$$\tau = \frac{\phi}{2} - 90°.$$

NOTE.—Equations (1) and (2) are due to Professor W. J. McAdam, Member of this Society.

APPENDIX B.—THE PARABOLA.

[By W. SCHERZER, Member of the Society.]

Given, the tangents OC and AB with their points of contact O and B, to determine the parabola.

Let the curve be referred to the rectangular axes OX and OY, O being a point of curve.

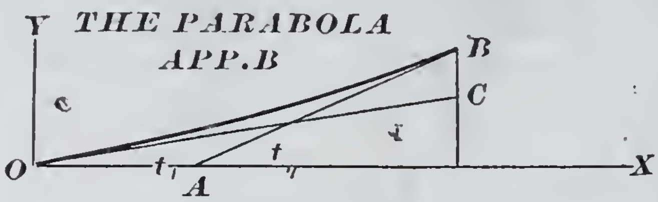
Let τ_i represent the angle formed by the tangent OC with the axis of X.
Let τ_{ii} " " " " " AB " " "

Let α and β represent co-ordinates of point B.

The co-ordinates of point O are equal to zero.

Let x and y represent the variable co-ordinates of the parabola.

Let (1) $x^2 + bxy + cy^2 + dx + ey = o$ be the equation of any conic section passing through the origin O, in which b, c, d and e are unknown constants to be determined in this special case from the data given above.



In order that the curve represented by equation (1) be a parabola, the well-known relation

$$(2)\ b^2 - 4\,c = o \text{ must exist.}$$

The equation of any tangent to the curve is

$$(3)\ \eta - y - \tan.\,\tau\,(\xi - x) = o$$

in which η and ξ are the variable co-ordinates of tangent, and τ the corresponding angle formed by the latter with the axis of abscissas.

$$(4)\ \tan.\,\tau = \frac{\delta\,y}{\delta\,x} = \frac{\eta - y}{\xi - x}$$

By differentiation we obtain from equation (1),

$$(2\,x + by + d)\,\delta x + (2\,c\,y + bx + e)\,\delta y = o,$$

then dividing by δx and substituting from equation (4) we obtain

$$(5)\ 2x + by + d + (2\,cy + bx + e)\tan.\,\tau = o,$$

the equation of the tangent at any point $(x\ y)$ of the curve.

For $x = o$, $y = o$, $\tau = \tau_{i}$, substituted in equation (5) we obtain

$$(6)\ d + e\tan.\,\tau_{i} = o.$$

For $x = \alpha$, $y = \beta$, $\tau = \tau_{ii}$ substituted in equation (5) we obtain

$$(7)\ (\beta + \alpha\tan.\,\tau_{ii})\,b + 2\,\beta\tan.\,\tau_{ii}\,c + d + \tan.\,\tau_{ii}\,e + 2\,\alpha = o,$$

and substituting in equation (1) we obtain

$$(8)\ \alpha\,\beta\,b + \beta^2\,c + \alpha\,d + \beta\,e + \alpha^2 = o.$$

We now have the four equations, viz: (2), (6), (7) and (8) from which the four unknown quantities b, c, d and e can be determined, and their values substituted in equation (1); by solving equation (1) we obtain,

$$(9)\ y = \frac{-(b\,x + e) \pm \sqrt{x\,(2e\,b - 4\,c\,d) + e^2}}{2\,c}$$

Example:—$\alpha = 800$ ft.; $\beta = 50$ ft.; $\tan.\,\tau_{i} = \frac{3.5}{100}$; $\tan.\,\tau_{ii} = \frac{15}{100}$. Substituting in equations (2), (6), (7), (8)

$$\begin{cases} b^2 - 4\,c = o \\ d + \frac{3.5}{100}\,e = o \\ (50 + 120)\,b + 15\,c + d + \frac{15}{100}\,e + 1600 = o \\ 40000\,b + 2500\,c + 800\,d + 50\,e + 640000 = o \end{cases}$$

By solving we obtain,

$$b = 112.94;\quad c = 3188.93;\quad e = -596822;\quad d = 20888.77$$

Substituting in equation (9) we obtain

$$y = \frac{-112.94\,x + 596822 \pm \sqrt{356191000000 - 401261800\,x}}{6377.86}$$

and from this equation we can find, for any value of x, the corresponding values of y.

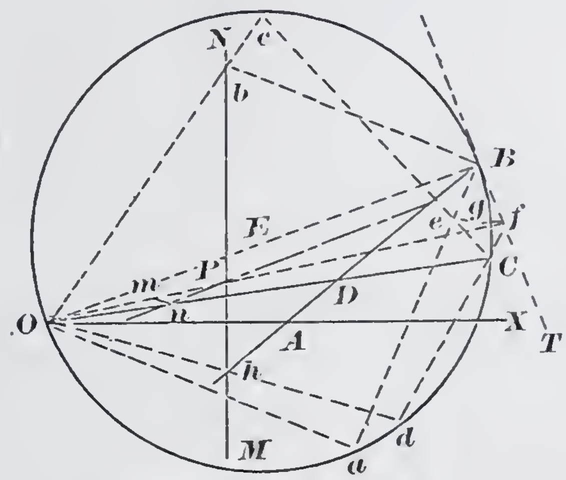
From equation (5) we obtain,

$$\tan.\ \tau = -\frac{2\,x + by + d}{2\,cy + bx + e} = -\frac{2\,x + 112.94\,y + 20888.77}{6377.86\,y + 112.94\,x - 596822}$$

From which can be found the angle τ formed by the tangent at any point $(x\ y)$, with the axis of x.

GRAPHICAL SOLUTION.

Let OX be a horizontal line. OC and AB are the given tangents, O and B their respective points of contact, the problem is to find the points of intersection of any line MN with the parabola, and the tangents at these points of intersection.



THE PARABOLA
Graphical Solution
APP. B

Solution:—Join O and B, bisect, and from centre E pass a circle through the points O and B. Draw Oa and Bb parallel to DE. Draw Ob and Oh to their intersection with circle in c and d respectively. Draw the tangent BT to circle, also join a and B. Draw Cc to its intersection e with aB, also draw Cd to its intersection f with the tangent BT; now join e and f and intersect circle at g. Draw Og and intersect MN in point P, which is the desired point of parabola. To find the tangent at point P, bisect PO giving point m. Draw mn parallel to DE then join Pn, which line is the desired tangent.

N. B. It is convenient to take the vertical scale five to 10 times greater than the horizontal scale. Results must be correspondingly reduced.

NOTE ON SOME CAUSES OF RED-SHORTNESS AND COLD-SHORTNESS IN IRON.

By W. Metcalf.

Paper read before the Engineers' Society of Western Pennsylvania March 16, 1883.

I do not propose to discuss the well known red-shortness and cold-shortness of iron caused by sulphur and phosphorus respectively, nor the well known effect of oxygen in producing red-shortness in steel. In fact, the latter is not altogether proven. For instance: In walking through an establishment with my partner, where some large steel ingots were being worked, we noticed that the ingots were being unevenly heated in a sharp oxydizing flame, and the corners were cracking badly and being chipped in every ingot. Upon calling attention to the matter, one of the gentlemen in charge replied: "Oh! that is not burnt; that's sulphur." As the gentleman had been in the business several months, we meekly closed our mouths. On the contrary, Mr. Park, who has been noted as a steelmaker for more than twenty years, charged all of the evils in steel to "wind," during a discussion before this society.

For the purpose of provoking a discussion, I have prepared a table of analyses, showing some odd results. After explaining the table I hope the members will explain the results.

No. 1 is an analysis of some Siemens direct iron, made at the Black Diamond Steel Works. The two tensiles given were obtained, the first from the iron once hammered and rolled, the second from the iron twice hammered and rolled. This iron was excessively red-short.

No. 2 is steel made from No. 1 with no addition to the iron except a little manganese, and whatever carbon the pot may have given it. This steel was not at all red-short; and you will observe by the tenacity that it was not cold-short.

No. 3 is some Dupuy direct iron. This was excessively red-short. Steel made from this iron of about .90 carbon, was not only not red-short, but it was remarkably strong and fine--the record says, quite equal to Jessup's Best, and Black Diamond, which we hap-

	Siemen's Direct.		Dupuy Direct.	United States Series.		Lucy.		Titan.	
	Iron.	Steel.		Mn.	Phos.	Pig.	Muck.	Pig.	Bar.
	1.	2.	3.	4.	5.	6.	7.	8.	9.
Silicon	.038	.004	.460		.048	.790	.142	2.17	.081
Phosphorus	trace.	.011	.010		.435	.090	.032	.054	trace.
Sulphur	.016	.016	.027		.002	.038	.003	.008	.015
Manganese	.000	.000		.694	.019				
Copper	.030	.02 Estim'te			.003			.000	.010
Carbon	.033	.300		2.36	.312				
Dissolved oxide	.300								
Oxides or cinder			.796				.113		
Tensile strength	38,556 40,824	87,400		Tried for.	Tried for.				
				Mn. 1.5 Carbon low.	Ph. .5 Carbon low.				

pened to be criticising about that time.

Nos. 4 and 5 are steels made for the United States Test Board.

No. 4 was an effort to produce a manganese steel containing 1 5 per cent of Mn., and carbon very low. You see how well we succeeded. This steel was very hot-short, and very cold short, and not at all red-short. This proves that the Bessemer men are all right when they go in for manganese and lively rail-rolling.

No. 5 was one of a phosphorus series we tried to make. In this case we aimed at P. .500 and C. low. We were not so wide of the mark this time. This steel hammered well at a good heat.

A piece $1\frac{1}{4}$ x $\frac{1}{8}$ inches, could not be hardened, and although little flakes could be knocked off easily with a hand hammer, the whole bar would bend cold to a right angle before fracture. It was not hot-short, nor cold-short to excess, notwithstanding an amount of phosphorus that, to say the least of it, was large.

No. 6 is Lucy Bessemer pig, and No. 7 is the resulting muck iron. These analyses will bear close inspection for their good qualities, and yet No. 7 was the most extremely red-short bar of iron I ever saw. It was for that reason that it was analyzed. As 2 per cent of cinder and .023 to .025 of sulphur are quite common in good neutral muck iron, this is the case of red-shortness I wish to have explained. No. 8 is Titan charcoal pig, and No. 9 is the resulting muck iron rolled down to $2\frac{1}{2}$ x $\frac{1}{2}$. This muck iron was very clean, granular, and highly lustrous in the fracture. It worked perfectly under the hammer, from welding to red heat. It looked hard, and I first thought it was a sort of puddled steel; but it proved to be dead soft: the more it was heated almost white and quenched, the softer it seemed to be. This bar was so cold-short that a piece a few inches long could be held in the fingers and crumbled to pieces on the anvil by light blows of a hand hammer. It was for this reason that it was analyzed, in a search for the cause of the cold-shortness.

DISCUSSION FOLLOWING PAPER READ BY W. METCALF, MARCH 16, 1883.

Proceedings of Engineers' Society of Western Pennsylvania.

James Park, Jr—Mr. President, in obedience to your request, to introduce the discussion upon the admirable paper of our former presiding officer, and to criticise the figures so fully placed in the table he has drawn on the blackboard, I must satisfy myself, by declining to satisfy the author of the paper, and the members of the society who are here this evening. Mr. Metcalf has ingeniously adopted his usual custom—which is useful to our membership—of forming his own opinions—which are usually the correct ones—in reference to problems he submits, but carefully withholding them, until the opportunity is given to all who have listened to him, to express their views or theories. When he finds a member on "the right track," towards a correct conclusion, you will notice he listens with close attention, and then corroborates the correctness of the member's views, adding to the confirmation of them, intelligent explanations, which are always useful to his listeners. I heartily approve of the course adopted by the author of the paper under discussion, as it is calculated to encourage all, who wish to do so, to express their theories, which would not be the case if the author gave his opinion in advance of the discussion. Fearing my theories may not be correct, I shall not express them, but leave the way open to others that it may be known how near they may be able to arrive at the true ones. Yet, I suppose it may be expected that I should say a few words about the "wind" theory, referred to by Mr. Metcalf, in which, we can infer, he has as strong faith as I have, as he, in a vein of pleasantry, shows he knew that it was not the fault of "sulphur," but that of the "wind," which made it necessary to chip the steel ingots, the heating of which he and his partner witnessed. The chipping became necessary, owing to the red-shortness, caused by the oxygen of the air having entered the heating furnace in sufficient quantity to oxydize the carbon of the steel, attacking the corners of the ingots, first, which proves, beyond dispute, that the honest but illiterate blacksmith, to whom I referred during a former discussion before this society, knew more than some of our learned metallurgists might be willing to give him credit for, when he expressed it as his opinion, and asked me if he was not correct, "that all the evils in working cast steel was chargeable to the 'wind.'" My answer being in the affirmative, he expressed much satisfaction at his theory being confirmed, and said, "you will never again hear of a piece of cast steel being spoiled or 'burned' in these shops." I am glad to be able to give evidence this evening, that up to this time the careful use of the "wind," under the instructions and direction of the honest but illiterate blacksmith referred to, has prevented the spoiling or "burning" of a single piece of steel in the shops of which he is in charge. I hope, Mr. President, that having a good attendance this evening, members may not hesitate in giving full expression to their views, as the subject brought to our attention is an interesting one, and deserving of full discussion.

Mr. Wm. Kent.—I am inclined to look with suspicion upon some of the figures in Mr. Metcalf's table of analyses. I cannot see how it is possible that Dupuy's direct process iron can contain so much as .460 of metallic silicon and at the same time contain .796 of oxides or cinder. That the Titan bar No. 9 should contain only a trace of phosphorus when the pig contained .054 is rather doubtful. I know of no puddling process which is so complete a dephosphorizer as this. The "dissolved oxygen" in Siemens's direct iron I think needs further explanation. It is very unusual to see "dissolved oxygen" mentioned in analyses of iron.

The red-shortness of the direct process irons is undoubtedly due to the presence of oxide of iron or partially reduced ore.

The reason the steels made from these irons are not red-short is that the oxides are removed by the melting process from the metal and float off in the slag, leaving the metal homogeneous.

The red-shortness of the Lucy muck bar I think is accounted for by the low silicon of the pig from which it was made. The low silicon causes the iron to "come to nature" too soon after it is melted, and before the puddler can ball it up. The action of the flame and the oxides in the bath is to cause too much oxydation of the metal, first the silicon is oxydized, then the carbon, then the iron, forming oxide of iron, which makes the bar red-short, as it does in the direct process iron.

If the silicon were in greater quantity or perhaps if the pig could be melted in a cupola before running into the puddling furnace, the too great oxydation might be prevented, and the ball removed from the furnace early enough to prevent the bar being red-short.

The cold-shortness of the Titan bar is a complete mystery, if it does not contain some elements not reported by the chemists which should cause its cold shortness. Such a sample is certainly a curiosity which should be given further study.

EVAPORATIVE TESTS OF STEAM BOILERS.

By William Kent, M. E.

Paper read before the Engineers' Society of Western Pennsylvania. April 15, 1883.

In a paper read more than a year ago by our ex-president, Mr. Metcalf, on "Some Wastes of Heat," he gave us an estimate in dollars and cents of the money annually thrown away in Allegheny county "by throwing money into our furnaces in the shape of coal, to be sent wasted out of the tops of stacks in the shape of dirty, useless smoke, and red, and far more expensive flames." His figures were taken from actual working data, and they referred to the waste in fuel and in scaling of iron in puddling and re-heating furnaces alone, and they footed up to the startling total of $1,063,537.37, which he says is the price we pay for keeping bonfires at the tops of our furnace stacks. These figures moreover do not represent the difference between the theoretical heating power of the coal burned and that actually obtained in practice, but only the actual loss due to perseverance in the use of the old style puddling and heating furnaces instead of substituting for them some good modern form of regenerative gas furnace. In other words, his estimate means that if the old style furnaces were abandoned and the new styles adopted, there would result an actual saving of $1,063,537.37 per annum.

There is another waste of fuel continually taking place in Allegheny county which perhaps does not foot up to quite such a large total in dollars and cents as the waste in puddling and heating furnaces, but it is more inexcusable, since it may be remedied at a much less cost than is involved in the introduction of regenerative gas furnaces for puddling and heating. I refer to the waste of fuel in producing steam power.

There are no such accurate statistics of the amount of steam power used in Allegheny county as there are of tonnage of iron products, which Mr. Metcalf used in his calculation, but I have made the following rough estimate of the cost of steam power in the county. A recent census shows that there are about fifteen hundred steam boilers in the county, nearly two-thirds of them being in our larger manufacturing establishments, and the remaining one-third in small establishments, using from one to three boilers each. A moderate estimate of the average power developed by each boiler is fifty horse power, making a total of 75,000 horse power. The coal used by these boilers is certainly not less than six pounds per horse power per hour, or say one bushel per day of twelve hours, costing on an average six cents per bushel, that is six cents per horse power per day, or $4500 per day for the 75,000 horse power. At 300 working days in the year the total cost of coal used for steam power in the county is $1,350,000. I think these figures are entirely within the mark, for a large number of our establishments run night and day, and the two-flue boiler, which is the almost universal boiler in the iron works, is generally driven at a rate of much more than 50 horse power. I think also that I am within the mark when I estimate that one-half of the cost of the total amount of fuel, or $675,000 per annum, is wasted, which might be saved by a correct application of what are elsewhere well known principles of modern steam engineering.

Of the fifty per cent of fuel wasted I estimate that twenty to thirty per cent is due to the perseverance of our manufacturers in the use of that respectable old relic of the days of the stage-coach, the two-flue boiler, and the driving of this boiler at a rate of nearly double its normal economical capacity, and the balance is due to the use of antiquated steam engines, to engines not properly proportioned to the work to be done by them, and to friction and condensation in long lines of steam pipes.

This is doubtless a severe and wholesale criticism of steam engineering methods of Pittsburgh and vicinity, and I may be asked to explain why this locality is so far behind the age in matters relating to the economical production of steam power. The answer is easy. The engineering of Pittsburgh in the line of iron and steel metallurgy is the very best in the world. It has the best blast furnaces, the best Bessemer and crucible steel works, the best rolling mills, it turns out the largest tonnage per furnace, and in everything relating to rapid production of heavy shapes of iron and steel it is nearly always found abreast of the best practice. The best engineering talent in the country has done its best work

on its great iron and steel furnaces and rolling mills, and has achieved the grandest results. On the other hand, but little talent or attention has been given to securing economy in cost of steam power.

There is possibly a good excuse for the past neglect of this economy, for the condition of success in an iron and steel works in the past has not been so much cheap cost of production as steady output of large tonnage. All the energies of the manufacturer and his subordinates have been necessarily directed to such improvements in his plant as would enable it to increase its tonnage of product. When there is ten dollars per ton profit on any certain iron or steel product, it is much more to the manufacturer's immediate advantage to double the tonnage, even at the expense of a reduced profit per ton, than it is to diminish the cost of production by two or three dollars per ton and make no increase in tonnage. But when trade becomes dull and the profit is cut down to two or three dollars per ton, then diminished cost of production is much more essential than increase of tonnage. It is to this latter condition, namely, one of small profits, that the iron and steel trade seems to be rapidly approaching. The necessity of economy in what have been heretofore considered minor matters will then be more apparent.

Another tendency of the times is to higher cost of fuel. It has been one of the boasts of Pittsburgh that its coal is cheap, but miners are getting much higher wages than they were getting five years ago, and the distance the coal has to be transported is gradually becoming greater, so that unless some means are taken to diminish the quantity of coal used in manufacturing, the coal bills of our iron firms will become a greater percentage of total cost of production than they have been in the past.

As the question "How shall we decrease our coal bills?" becomes worth considering by our manufacturers they will naturally inquire how they shall find out where the greatest wastes of fuel take place, and which are most easily remedied. The purpose of this paper is to give at least a partial answer to the inquiry.

The value of a correct system of book-keeping is now generally understood in all large manufactories. In earlier times accounts were kept by cutting notches on a stick, and the balance of cash was discovered simply by counting how much was on hand. Now the cash account is kept in a cash book and every penny has to be accounted for in a written record. Every man's wages are paid to the exact penny the pay roll calls for. No one would dream of throwing out the wages to the men by the handful and keeping no record, but many of our manufacturers pay men, whose wages are figured down to a penny, to throw away coal costing thousands of dollars per annum, a shovelful at a time, into heating and puddling furnaces and under steam boilers, and keep no record whatever except the record of payment of the monthly coal bills. The first step necessary to securing economy of coal is to know where the waste is, and to know this the first step is to apply the principles of book-keeping and of weighing and measuring to the coal used for various purposes, just as they are applied to the cash in the drawer and to the iron and steel in the warehouse and shipping room.

An evaporative test of a steam boiler, reduced to its simplest terms, consists mainly in finding out how much work is done by each pound of coal burned. The work done by a steam boiler is to turn water into steam; the economy of the work is directly proportional to the number of pounds of water turned into steam by one pound of coal. To test the economy of a steam boiler then is to weigh the coal used and to weigh the water evaporated, and to divide the weight of water by the weight of coal. The higher the quotient the greater the economy.

One pound of pure carbon, thoroughly burned to carbonic acid, will generate 14,500 heat units, and if this number of heat units could be entirely absorbed by the water in a boiler with no loss by radiation and no escape of heat up the chimney, they would evaporate 15 pounds of water at 212° F. into steam at the pressure of the atmosphere, or about 12.55 pounds from a feed water temperature of 60 degrees into steam at 100 pounds gauge pressure. As it is a very fair coal which is equal in heating power to 90 per cent of its weight of pure carbon (the 10 per cent loss being ashes and moisture,) such a coal would evaporate 13½ lb. of water from 212° to steam at 212°, or 11.3 lb. of water from 60 degrees to steam at 100 lb. pressure. This is the best result theoretically possible, assuming no radiation and no escape of heat into the chimney. As it is rarely that this loss from radiation and from heated gases in the chimney is less than 25 per cent of the total heat generated, the actual practicable evaporation from one pound of such a coal is about 10 pounds of water from and at 212°, or 8.47 lb. from 60° to 100 lb. steam pressure. This result, 10 pounds of water at 212° into steam at the same temperature is what may always be obtained from a pound of good coal under good conditions. Anything less than this indicates either bad coal, improper setting of furnace and boiler, unskillful firing, too slow or too rapid combustion, a bad type of boiler, or one too small for the work, a boiler with scale or other deposit on the inside or outside of the heating surfaces, excessive escape of heat by radiation or up the chimney, indraughts of cold air into the furnace or flues, or perhaps several of these conditions at once. To show what a range of economy may be obtained with varying

conditions, I may instance some tests I recently made. With the same kind of a boiler, at different places, I obtained an evaporation of 10.4 pounds of water per pound of coal, and less than 4 pounds. The conditions of course were very different. The coal in the first case was good Pittsburgh lump containing 9 per cent of refuse, in the second it was Illinois coal containing over 20 per cent refuse. The boilers were of the same type in each case, both clean and well kept, but the furnaces were different. In the first case the furnace was probably the best possible for the conditions, in the second the furnace had been improved (?) by the resident engineer, and was said by him to be much better than it was originally. In the test which gave less than 4 pounds evaporation, moreover, the coal was evidently burned too rapidly for the best results, for it clinkered so badly as to choke the grates within one hour after they had been cleaned. With slower combustion this coal gave at least twenty-five per cent better results. A carload of it will be shipped within a few days to another point where it can be tested under the same kind of a boiler with a different furnace setting, and I have no doubt still better results will be obtained. This is a fair sample of the range of economy which may be obtained with the same boiler under different conditions.

A still more instructive case perhaps is one I recently made in Pittsburgh where the boilers were of different types and the coal and other conditions as nearly alike as they could be made without interfering with the regular work of the mill. The coal was from Castle Shannon, a mixture of screened lump and nut, it was very free burning, made but little clinker, and a sample contained 11 per cent of ash. The test of one set of boilers was made one week, from Monday morning to Saturday night, and the other set the next week from Monday morning to Wednesday afternoon. The results showed for one set 9.709 pounds of water per pound of coal, and for the other set 6.334 pounds, both being reduced to the equivalent evaporation from and at 212°, showing a saving in favor of one set of boilers over the other of 34 76 per cent. Both sets of boilers were driven beyond their normal rate, the first set being Babcock & Wilcox boilers rated at 416 horse power, developing 522 horse power on an average for the whole week, and the other set were eight two-flue boilers, rated at about 320 horse power but actually developing 741 horse power. The waste of fuel in the two-flue boilers was confirmed by measuring the temperature of the gases escaping in the chimneys by means of a pyrometer. In a half-hour test the temperature of the chimney gases with the water tube boilers varied from 420° to 460°, while in the two-flue boilers it varied from 830° to 1000°. Calculating the works to run 300 days of 20 hours each, or 6000 working hours per year, and the cost of coal 6 cents per bushel, the 34 76 per cent wasted by these eight ton flue boilers figures up to $7278 per year. Since there are nearly a thousand such boilers in Allegheny county, and it is not customary to drive boilers at a slow rate, or to take any pains whatever to economize fuel under them, the total loss of fuel uselessly escaping up their chimneys may easily be estimated to be nearly equal to the loss estimated by Mr. Metcalf for puddling and heating furnaces.

Since I have used the term "horse power" in the above remarks, and there is frequently a difference of opinion as to what a horse power is, I will define it to be 30 pounds of water evaporated per hour from 212° into steam at 70-lb. pressure. This was the standard adopted by the judges at the Centennial exhibition, it has been generally accepted by engineers since, and is about the amount of water that should be evaporated to develop one indicated horse power in a good non-condensing engine fairly proportioned to its work. A two-flue boiler should have about 10 square feet of heating surface and a tubular boiler about 15 square feet to develop a horse power with a fair economy of fuel. In practice they are often used to develop a horse power from only one half, or less, of this amount of heating surface, and they do it at a great sacrifice of economy and of durability. This is perhaps the chief cause of the waste of fuel by the two-flue boiler. Its first cost is so high and it takes so much room in the mill, that most manufacturers would rather waste fuel by driving it beyond its capacity than to double the number of boilers as they should do to get reasonable economy out of them, especially if they do not know what the amount of waste is, since they have never gone to the trouble of having it measured.

I will now proceed to the practical details of the method of making an evaporative test. It can easily be made by any intelligent chief engineer of a works, and there is no reason why a manufacturer should not require such a test made occasionally to discover whether or not the boilers are being run economically, any more than there is why he should not periodically require a balance sheet to be taken from his books.

The easiest way to make a test is to pass the feed-water through a water meter, and to have a record of the weight of the coal kept as it is delivered to the boiler room. A record of the steam pressure and of the temperature of the feed should be kept during the test, and it would be advisable also to have the ashes weighed, so as to have a correct idea of the quality of the coal. The weight of the water divided by the weight of the coal gives the evaporation under actual conditions. By a simple calculation

hereafter explained, the figures thus obtained are reduced to the equivalent evaporation from and at 212 degrees, which is a standard by which all tests may be compared.

Such a test as this may be made without interfering in any way with the regular running of the works. The water meter may be connected to the feed pipe during the dinner hour, and it will register the water used for a week or a month if desired without any attention whatever. A record of each load of coal delivered to the boilers may be kept at the scale house. A comparison of the figures registered by the water meter during any interval of time will indicate the amount of power the boiler is developing, and an inspection of the coal record for the same time will show how much it costs to obtain this power. If the quotient obtained by dividing the weight of water by the weight of coal is from 8 to 10 it is an indication that the boiler is doing fairly well, if it is only from 5 to 7 and the coal is of fair quality, it shows that there is a serious loss of fuel somewhere which should have immediate investigation.

Where it is important to secure great accuracy in a test, as in a competitive test between two boiler makers, or to determine whether or not a guarantee has been fulfilled, several precautions are necessary. If a water meter test is decided upon the water meter should be tested to determine whether it correctly registers the water passing through it, or if a tank or set of barrels is used to measure the water the capacity of such tank or barrels should be carefully weighed or measured, and an accurate count kept of the number of times they are filled and emptied. The scales upon which the coal is weighed should also be tested, and the number of drafts weighed carefully checked. The boiler should be inspected for leaks and any water blown off through the mud valve should be caught and weighed. To have the conditions fairly in favor of the boiler, it should be cleaned inside and out before the test, the brick work should be examined to insure that there are no indraughts of air, the coal should be of a quality ascertained and agreed upon, and there should be a skillful fireman accustomed to the working of the same coal under the same kind of a boiler. The amount of coal upon the grate and the condition of the fire should be the same at the end of the test as at the beginning, the water in the boiler should be at the same level and the steam at the same pressure. All of these precautions are quite easily observed, and should be within the comprehension of every one who is entrusted with the care of a boiler. There is nothing fanciful nor theoretical about them.

As there is a considerable chance for an error of judgment in estimating the amount of coal on the grates and the condition of the fire at the beginning and at the end of a test, it is customary in accurate tests to raise steam in the boiler before beginning the test, and to have the brick work thoroughly tested as when the boiler is in its best working condition, then to draw the fires, removing all fuel and ashes from the grates and ash pit. A fresh fire is at once kindled with wood and shavings (the weight of which is taken as equivalent to 4 10 of the same weight of coal) and with weighed coal. At the end of the test, when the fire is in as nearly as possible the same condition as the fire which was drawn before the test, it is drawn and the grates and ash pit cleaned, and the fuel which was on the grate cooled with water screened and picked to separate the coal from the ashes, and the coal and the ashes are weighed separately. In this manner the actual amount of coal used to evaporate a certain amount of water is much more accurately obtained than when the amount of fuel on the grate at the beginning and end of the test is merely estimated.

When still greater refinement is desired it may be necessary to test the quality of the steam escaping from the boiler, that is, to determine whether it is dry saturated, superheated, or whether it contains "entrained" water, or priming. A good boiler, properly proportioned, and not driven beyond its normal rating, and with good feed-water, when there is no provision made for superheating, should give practically dry steam, that is, not containing above 3 per cent of moisture. If there is a superheating surface attached to the boiler, the steam may not only be perfectly dry, but heated to a temperature considerably above the temperature due to its pressure, while with a boiler having insufficient water surface, insufficient steam space, certain kinds of bad feed water, or which is driven too hard, the amount of water carried over with the steam may be ten or fifteen per cent.

To determine the percentage of water carried out of a boiler in the steam is a matter requiring considerable skill and extremely fine instruments for weighing and for measuring temperatures. The operation is such a delicate one that there is considerable doubt of the accuracy of many priming tests which have been published, including among others those made at the Centennial Exposition. The method of determining the percentage of moisture in the steam consists in comparing the amount of heat contained in the mingled steam and water, (as measured by the increase in temperature given by a certain weight of it to a certain other and larger weight of cold water) with the heat which would be contained in an equal weight of dry saturated steam. One plan of making this comparison, which was adopted at the tests at the American Institute in New York in 1871, is to condense all of the steam made by

the boiler during the test by means of a surface condenser, and to determine its weight, and the weight and increase of temperature of the condensing water. Such a test is very expensive and a difficult one to make, and it is not often attempted. A much more common plan is to take samples from the steam at intervals, say about ten pounds each test, and pass it directly into a barrel containing a known weight, say 200 pounds of cold water. The increase in temperature of this weight of water supplies the data which are inserted in a formula hereafter given, and the solution of the equation thus formed gives the percentage of dry saturated steam in that which escapes from the boiler. To obtain reasonably accurate results with this apparatus it is necessary to have a scale for weighing the barrel with the water in it to within one-tenth of a pound, and a thermometer giving readings accurately to a tenth of a degree.

Probabiy the most accurate apparatus for determining the quality of steam is the calorimeter of Mr. J. C. Hoadley, described by him in a paper in a Comparative Test of Boilers, published in *Van Nostrand's Engineering Magazine*, in December, 1882. It consists of a cylindrical vessel of 24 oz. tinned copper, encased in a jacket of heavy galvanized iron, the space between the outer and inner vessels all around and at the bottom being filled with two layers each $\frac{3}{4}$ inch thick of eider down surrounded by a $\frac{3}{4}$ inch layer of hair felt. It has a cover of similar construction, thickness and filling. The vessel is charged for experiment with 200 pounds of water. The steam drawn from the boiler, instead of being discharged directly into the water is condensed in a tubular copper drum forming a surface condenser, which is placed inside of the calorimeter. Above the condenser is placed a two-bladed propeller attached to a shaft and crank, by means of which the 200 pounds of water surrounding the condenser is agitated in order to equalize its temperature. The condensed steam kept entirely separate from the water in the calorimeter is drawn off from the condenser and weighed on a delicate balance. The thermometer by which the increase of temperature of the water in the calorimeter is measured is so delicate as to admit of accurate readings to 1-100th of a degree. A test recorded by Mr. Hoadley shows that 6.3 pounds of steam at a pressure of 101.7 pounds elevated the temperature of the water in the calorimeter from 44.98 degrees to 77.68 degrees, a rise of 32.7 degrees. The calculation from these data showed that the steam contained 3.15 per cent of entrained water. Mr. John W. Nystrom, by another apparatus, which is not described by Mr. Hoadley, but which the latter says is correct in principle but hardly admits of the degree of accuracy obtained by Mr. Hoadley's calorimeter, obtained only about half of this percentage.

Other refinements which may be introduced into a boiler test are measurements of the temperature of the escaping gases, of the force of the draught, of the temperature of the fire, analyses of the fuel and of the heated gases, and measurement of the quantity of air supplied. In the test by Mr. Hoadley before referred to, the temperature of the escaping gases and of the fire were measured, and an analysis was made of the amount of carbonic acid in the flue gases. From these data, he calculated that of the total heating power of the combustible (that is coal after deducting ash and moisture) 20.54 per cent was carried up the chimney by the heated gases. The water in the boiler absorbed 75.03 per cent and 4.48 per cent was lost by radiation. The temperature of the waste gases in this case averaged was 467° F., and the analysis showed them to contain 11.34 per cent by weight of carbonic acid.

The use of the pyrometer to measure the temperature of the waste gases furnishes an important check in many cases of the records obtained by weighing the water and the coal in a boiler test. As in the comparative test of the two-flue and the water-tube boilers already referred to, the high temperature of the pyrometer, 830° to 1000° is alone sufficient proof of the great waste of fuel in the two-flue boilers. When a boiler is working with fair economy the temperature of the gases in the flue should not be over 500°. A low temperature of the gases however does not of itself indicate economical working of the boiler, for it may be caused by too great a supply air to the fire, diluting the waste gases and reducing the temperature of the fire, or too small a supply of air, causing the burning of the fuel to carbonic oxide only, with the generation of only one-third of the heat which would be generated by its thorough combustion into carbonic acid. It may also be caused by indraughts of air through the setting of the boiler between the grate and the flue. Occasionally a pyrometer will show a temperatue below that of the steam in the boiler. This is almost certain proof of the existence of indraught of air.

The force of the draught, or the difference between the pressure of the outside atmosphere and that of the heated gases in the chimney flue is measured by the draught gauge, which is merely a U-shaped glass tube partially filled with water, one leg connected by a rubber or other tube to the inside of the flue, and the other exposed to the outside air. The partial vacuum in the chimney will cause the level of the water in the leg connected with the interior of the chimney to rise and that in the other leg to fall. The difference of the water level is a measure of the force of the draught. An ordinary draught for a chimney to a steam boiler is from $\frac{1}{4}$ inch to 1 inch, the amount of draught needed varying largely with different set-

tings of boilers and with different fuels. The draught gauge is not a necessary part of the apparatus required in a boiler test, but its indications are often useful in comparisons of the working of one boiler with another, and it sometimes indicates the cause of the lack of economy or of capacity of any given boiler. At a test at which I was present a few months ago there were two boilers connected with the same chimney. The evaporative test showed that one boiler was giving a much larger capacity than the other, which was supposed to be equal in every respect to the former. The draught gauge showed that the slow working boiler had a draught of only 3-16 of an inch water column, while the other had $\frac{5}{8}$ inch, and indicated that an improvement could be made by altering the chimney connection, so that its draught would not be interfered with by that of the other boiler.

Determinations of the temperature of the fire, by means of the platinum pyrometer, as described in the paper by Mr. Hoadley, analyses of the fuel and of the heated gases, and measurements of the air supply are not often attempted in boiler tests, but they often furnish valuable data in experiments with new forms of boiler furnaces, with unusual fuels, or in scientific researches. The analysis of the escaping gases would be very valuable in tests designed to improve economy, for the percentage of carbonic acid in the gases is the most accurate measure of the thoroughness of combustion.

Having completed the boiler test, and obtained the three principal data, pounds of coal used, pounds of ashes and refuse, and pounds of water, the calculations now have to be made to reduce the results of the test to a standard for comparison with other tests.

The quotient of the weight of water divided by the coal gives what is called the evaporation per pound of coal under actual conditions, which conditions are stated to be the average temperature of the feed water and the average pressure of the steam. Dividing the evaporation per pound of coal by the ratio the combustible portion of the coal bears to its total weight, we obtain the evaporation per pound of combustible. These results have now to be reduced to the equivalent evaporation per pound of coal, and per pound of combustible "from and at 212°," (that is, the equivalent number of pounds of water which would have been evaporated if the feed water had been supplied at 212° and the steam evaporated at ordinary atmospheric pressure at the sea level, or 14.7 pounds per square inch), and to do this we have to multiply them by a "factor of evaporation" which we obtain as follows:

In Nystrom's Pocket Book of Mechanics and Engineering there are two tables entitled Properties of Water and Properties of Steam, which were calculated by Mr. Nystrom from the formulae deduced from Regnault's experiments. Select from the Steam Table, column H, the Total Heat Units above 32° per pound of steam at the average observed gauge pressure, and from the Water Table, column h, the total heat units above 32° per pound of water at the observed average temperature of the feed. Subtract the latter from the former, and divide by 965.7, which is the latent heat of steam at 212°, and the quotient will be the "factor of evaporation" by which factor the evaporation per pound of coal and per pound of combustible under actual conditions is to be multiplied to give the equivalent evaporation from and at 212°.

In an appendix to this paper I have given a table for convenient reference containing the figures for total heat of water and steam at various temperatures, which is condensed from Nystrom's tables. I have also given a table of factors of evaporation for the feed temperatures and steam pressures used in common practice, which are calculated from the formula

$$F = \frac{H-h}{965.7}$$

in which F is the factor of evaporation, H is the total heat above 32° of the steam at the observed gauge pressure, and h the total heat above 32° in the water at the observed temperature of feed.

If a priming test is made to determine the quality of the steam, the following data and formula are used:

Let W = original weight of water in calorimeter.
t = " " total heat ℔ ℔ of water in calorimeter.
t' = final do. do. do.
w = weight of steam and water added from boiler.
H = total heat of steam at observed pressure.
h = " " water " "
Q = quality of steam, dry saturated steam being unity.

$$Q = \frac{1}{H-h}\left[\frac{W}{w}(t'-t)-(h-t')\right]$$

If $Q < 1$, then *moisture* $= 100\,(1-Q)$
If $Q > 1$, then superheating in degrees $= 2.0833\,(H-h)\,(Q-1)$

The figures for total heat of water and steam, H, h, t, and t^1 are all taken from the steam and water table, using the original and final temperatures of the water in the calorimeter and the pressure of steam as data.

Another form of the priming formula is

$$w'' = \frac{w\,H + Wt - (W+w)\,t'}{H-h}$$

in which w'' is the weight of dry steam contained in the mingled steam and water w drawn from the boiler.

When priming tests are made, a large number should be made at regular intervals during the progress of the test, and the results averaged, in order to eliminate the effect of certain unavoidable errors of observation, and of the differences in quality of the steam at different times caused by more

rapid evaporation and consequent greater disturbance of the water surface at one time than at another. For a further discussion of the subject of priming tests I may refer those interested to the Reports of the Judges of the Centennial Exhibition, Group XX. pages 81 and 136, in which some of the precautions necessary to insure accuracy are described. A study of the priming tests of the fourteen boilers tested at the Exhibition which are given in this report shows some curious results. Two tests were made of each boiler, the first called the capacity test, in which the boilers were fired with full draught, and the second the economy test, in which the draught was checked so that the fires burned only about three-fourths as much coal as in the capacity test. According to the figures, two of the boilers superheated when driven fast and primed when driven slow, two primed when fast, and superheated when slow driven, five primed more when fast than when slow driven, and one primed less, three superheated more, and two superheated less when fast than when slow driven. The Galloway boiler, which was tested both with anthracite and with bituminous coal, primed in both tests with anthracite, and superheated in both tests with bituminous. These results are in many cases so different from what might have been expected as to throw serious doubt on the accuracy of the tests themselves. The total range in quality of the steam was from over 18 per cent of priming to 71 degrees of superheating. Only three of the boilers tested showed a priming of over three per cent, and the probable error of the test itself is supposed by good authority to be equal to this percentage. This all tends to show that figures obtained from priming tests must be received with some degree of allowance.

It would be very desirable for comparing records of tests of different boilers if engineers would agree upon some uniform method of reporting results. I have had frequent occasion to study the reports of tests, in which some of the data had been left out I give below a schedule of headings which has been found convenient in reporting tests, and the data under each heading should be filled out whenever possible.

1 Date of trial.
2 Description of boiler, including square feet of grate and heating sufaces.
3 Kind of fuel.
4 Duration of test.
5 Average steam pressure.
6 Average temperature of feed.
7 Pounds of coal burned.
8 Percentage of ash and moisture in coal.
9 Pounds of combustible in coal.
10 Coal consumed per square foot of grate per hour.
11 Water evaporated, pounds.
12 " " ℔ sq. ft. of heating surface per hour.
13 " " ℔ ℔. of coal, actual conditions.
14 " " " " from and at 212°.
15 " " " of combustible, actual conditions
16 " " " " from and at 212°.
17 Rated horse power.
18 Horse power developed (at 30 ℔. water per hour from 212° to 70 ℔. pressure).
19 Horse power per cent above (or below) rated capacity.
20 Temperature of flue gases, by Pyrometer.
21 Force of draught, in inches of water column.

The horse power developed is obtained by taking the number of pounds of water evaporated per hour under actual conditions, reducing this by means of the steam table and formula heretofore given (or by multiplying by the factor of evaporation), to the equivalent evaporation from and at 212°, dividing the product by 1.033 (which is the ratio of the evaporation of 1 lb. from and at 212° to the evaporation from 212° to steam at 70 lb. pressure) and by 30.

After having made a test of a boiler, and having discovered that the economy of fuel is not what it should be, the next thing for the owner of the boiler or his engineer to consider is what are the causes which lead to a waste of fuel. In order for a proper consideration of this question it is essential to have a knowledge of the conditions necessary to secure economy. A few words on this subject may here not be out of place.

Briefly stated, the chief general conditions upon which economy of fuel in a steam boiler depends are first the complete combustion of the coal with the smallest possible supply of air which will burn all the carbon of the coal to carbonic acid; second, the carrying into the chimney of the smallest volume of gases at the lowest possible temperature, which temperature, however, must not be lower than the temperature of the steam in the boiler, and third, the smallest possible escape of heat by radiation.

Complete combustion of the coal with the minimum supply of air is coincident with high temperature in the furnace. The temperature may be lowered by too great a supply of air either through the grate bars or above them, also by too small a supply, causing the coal to be only partially burned, allowing the escape of smoke and of unconsumed carburetted hydrogen and carbonic oxide gases. To secure complete combustion with a minimum supply of air two things are necessary, a good furnace and a good fireman. It is unfortunate that machinery has not yet taken the place of the severe manual labor of shoveling coal into a furnace, and at least twenty per cent of the fuel with the best furnace and the best boiler may be wasted by a bad fireman which might be saved by a good one. A good fireman is not generally appreciated nor as well paid as he should be, for by his skill he may easily save to his employer as much money as his wages amount to.

If there is a good fireman and a good furnace, and the coal is thoroughly burned, the next place to look for waste is in the boiler itself. In the first place the setting of the boiler, its flues, and other passages of the heated gases should be so arranged that the

currents of the gases will be caused to be thoroughly split up into small portions and to hug, as it were, the heating surfaces of the boiler, instead of being allowed to find short passages of exit to the chimney along the walls, where only one side of a current has a chance to touch the heating surface. The arrangement and position of the heating surface with reference to the position of the furnace and the direction of the currents of gas are thus of primary importance.

The thinness of heating surface is another condition of economy. It has been experimentally proved that heat will pass quicker through a thin plate of iron into water on the other side of it than through a thick one. So also the freedom from scale is a still more important condition. Some experiments have indicated that 1-16 of an inch of scale will cause a loss of 13 per cent of fuel, $\frac{1}{4}$ inch 38 per cent, and $\frac{1}{2}$ inch 60 per cent. The keeping of the inside of a boiler perfectly clean is therefore of the greatest importance in order to avoid waste of fuel, as it also is to avoid danger of explosion.

The next condition of economy of fuel is the extent of heating surface. Other conditions being the same, the greater the extent of heating surface, the more heat will be absorbed from the heated gases, the lower temperature these will have when they reach the chimney, and the greater will be the economy. Other conditions being the same the smaller the heating surface, the less the boiler will cost, but the more it will waste fuel. There may be circumstances under which the interest on first cost of increased heating surface, and the value of the room occupied by it will be greater than the saving of fuel which might be made by such increase, but in ordinary practice, especially in Pittsburgh, it will generally be found that the saving of fuel would more than repay the extra expense of increase of heating surface.

Mr. Chas. E. Emery, C. E., in the Report of the Judges of the Centennial Exhibition, before referred to, gives a table showing the ratio of the capacity and economy to the extent of heating surface, which I have reproduced in the appendix to this paper. It is based on the highest possible rate of evaporation for the several rates of combustion, assuming the water to be evaporated from a temperature of 212° into steam at atmospheric pressure, that 24 pounds of air are used to burn each pound of combustible, and that the gases escape into the chimney at 212°. This table shows conclusively the great loss of economy due to driving boilers too rapidly, or trying to get more than a reasonable evaporation per square foot of heating surface. For instance, with 25 square feet of surface per horse power, a horse power may be obtained with 3.02 pounds of coal, and with 9 square feet, with 3.23 pounds of coal, which indicates that to increase the heating surface from 9 square feet to 25 square feet would scarcely pay for the small saving of coal, but if only 4 square feet per horse power are used, which is not uncommon in this vicinity, the coal consumption rises to 3.77 pounds, a waste of 15 per cent. If a boiler owner will figure up the heating surface of his boiler, and the amount of coal burned per square foot of heating surface per hour, this table will show what relative economy may be expected from such a rate of combustion per square foot of heating surface, and it will generally be found that if his boiler burns more than from 3 to 4 tenths of a pound of coal per square foot of heating surface, unless he gets his coal for almost nothing, that it will pay to put in an extra boiler, or change the one he has for a larger one. It must be remembered that the figures in the table of water evaporated per pound of combustible and coal per horse-power per hour represent the best possible results, and they will rarely be reached in every-day practice.

Another condition of economy of fuel in a steam boiler is the circulation of the water within it. As the efficiency of any portion of the heating surface, or the rate of transfer of heat through it is greater the greater the difference between the temperature of the water inside and of the heated gases outside, consequently if the circulation of the water inside is of such a nature as to continually sweep the hotter portion of the surface with fresh currents of water, it will render the heating surface more effective.

The conditions of economy, however, are all reduced to the three first mentioned, namely, proper combustion of the fuel, so as to obtain the highest temperature of the fire, the least loss of heat up the chimney, and the least loss from radiation.

The principles of the proper proportioning of steam boilers are now well understood among engineers who have devoted attention to the subject. It is possible in regular practice, with a good boiler, a good furnace under it and a good fireman to utilize 75 or 80 per cent of the theoretical heating power of the combustible. There is, therefore, but little margin for improvement in the construction of the best makes of steam boilers in order to secure any greater economy. What is needed is the application of well known principles of economy to every-day practice. The furnace under the boiler is in need of improvement much more than the boiler itself. A furnace for burning bituminous coal without smoke and at the same time without excess of air supply, and without a steam jet, which shall be fed by machinery, or automatically by gravity, and is not dependent for its economical results upon the skill of some poorly paid fireman is among the possibilities of the future. It may be that we shall come to gas firing, using some form of gas producer which is not yet before

the public. Improvements in this direction are greatly needed, and the fortunate inventor who brings them into general use will reap a rich reward.

APPENDIX.

TABLE I.

HEAT UNITS IN WATER.

Temp. Fahr.	Water. Heat Units. Per lb.	Temp. Fahr.	Water. Heat Units. Per lb.	Temp. Fahr.	Water. Heat Units. Per lb.	Temp. Fahr.	Water. Heat Units. Per lb.
32°	0	62	30.01	92	60.05	122	90.14
33	1	63	31.01	93	61.05	123	91.15
34	2	64	32.01	94	62.05	124	92.15
35	3	65	33.01	95	63.05	125	93.15
36	4	66	34.01	96	64.06	126	94.16
37	5	67	35.01	97	65.06	127	95.16
38	6	68	36.01	98	66.06	128	96.17
39	7	69	37.01	99	67.06	129	97.17
40	8	70	38.01	100	68.07	130	98.18
41	9	71	39.02	101	69.07	131	99.18
42	10	72	40.02	102	70.07	135	103.20
43	11	73	41.02	103	71.08	140	108.23
44	12	74	42.02	104	72.08	145	113.26
45	13	75	43.02	105	73.08	150	118.29
46	14	76	44.02	106	74.08	155	123.33
47	15	77	45.02	107	75.09	160	128.36
48	16	78	46.02	108	76.09	165	133.40
49	17	79	47.02	109	77.09	170	138.44
50	18	80	48.03	110	78.10	175	143.49
51	19	81	49.03	111	79.10	180	148.54
52	20	82	50.03	112	80.10	185	153.58
53	21	83	51.03	113	81.11	190	158.64
54	22	84	52.03	114	82.11	195	163.69
55	23	85	53.03	115	83.11	200	168.75
56	24	86	54.04	116	84.12	205	173.81
57	25	87	55.04	117	85.12	210	178.87
58	26.01	88	56.04	118	86.13	212	180.90
59	27.01	89	57.04	119	87.13		
60	28.01	90	58.04	120	88.13		
61	29.01	91	59.05	121	89.14		

[DEFINITION.—The British heat unit is the quantity of heat required to elevate the temperature of one pound of distilled water from 39° to 40° F. The heat units in these tables are the differences between the amounts of heat in water and in steam at the temperatures opposite each and the amount in water at 32° F.

TABLE II.

HEAT UNITS IN WATER AND IN STEAM.

[Condensed from Nystrom's "Pocket Book."]

Gauge Pressure lbs.	Temp. Fahr.	Water. Heat units, per lb. *h.*	Steam. Heat units, per lb. *H.*	Gauge Pressure, lbs.	Temp. Fahr.,	Water. Heat units, per lb. *h.*	Steam. Heat units, per lb *H.*
30+	274.33	244.32	1165.6	69+	315.25	286.27	1178.1
31	275.68	245.70	1166.0	70	316.08	287.12	1178 3
32	277.01	247.06	1166.4	71	316.90	287.96	1178.6
33	278.32	248.40	1166.8	72	317.71	288.80	1178.8
34	279.62	249.73	1167.2	73	318.51	289.62	1179.1
35	280.89	251.03	1167 6	74	319.31	290.44	1179.3
36	282.14	252.30	1167.9	75	320 10	291 26	1179 6
37	283.39	253.58	1168 4	76	320.88	292.06	1179.8
38	284 58	254.80	1168.7	77	321 66	292.85	1180.
39	285.76	256.01	1169.	78	322.42	293.65	1180.3
40	286.96	257.24	1169.4	79	323.18	294.43	1180.5
41	288.09	258 38	1169.8	80	323 94	295.21	1180.7
42	289.24	259.67	1170 1	81	324.67	295.96	1180.9
43	290.37	260.71	1170.5	82	325.43	296 75	1181.2
44	291 48	261.87	1170.8	83	326.17	297 51	1181.4
45	292.58	262.99	1171.2	84	326.90	298.26	1181.6
46	293.66	264.10	1171.5	85	327 63	299.01	1181 9
47	294.73	265.20	1171 8	86	328.35	299.75	1182.1
48	295.78	266.27	1172.1	87	329.07	300.50	1182.3
49	296.82	267.34	1172.5	88	329 78	301.23	1182.5
50	297.84	268.39	1172.8	89	330 48	301.95	1182.7
51	298.85	269.42	1173.1	90	331.18	302.67	1182.9
52	299.85	270.45	1173.4	91	331.87	303.38	1183 2
53	300.84	271.46	1173.7	92	332.56	304.10	1183 4
54	301.81	272.46	1174.	93	333.24	304.80	1183 6
55	302.77	273.44	1174.3	94	333.92	305.50	1183.8
56	303.72	274.42	1174 6	95	334.59	306.19	1183.9
57	304.69	275.40	1174 9	96	335.26	306.88	1184.2
58	305 60	276.35	1175 1	98	336.58	308.34	1184.6
59	306.52	277.30	1175.4	99	337.23	308.91	1184.8
60	307.42	278.22	1175.8	100	337.89	309.60	1185.
61	308.32	279.14	1176.	105	341.0	312.87	1185.9
62	309.22	280 07	1176.2	110	344.1	316.04	1186.9
63	310.11	280.98	1176.5	115	347.1	319.12	1187.8
64	310.99	281.87	1176 8	120	350.0	322.13	1188.7
65	311.86	282.78	1177.	125	352.8	325 06	1189.5
66	312.72	283.66	1177.3	130	355.6	327.91	1190.4
67	313.57	284.54	1177.6	135	358.4	330.75	1191 2
68	314 42	285.41	1177.9				

NOTE.—To the gauge pressure in this table should be added the fraction 0.3125, since the table was calculated for absolute pressures, 14.6875 lbs. greater than gauge pressures.

TABLE III.

FACTORS OF EVAPORATION.

Feed Water Temperatures.

Gauge pressure.	32	36	40	45	50	55	60	65	70	75	80	85	90	95	100	105	110	115	120
60........	1.218	1.213	1.209	1.204	1.199	1.194	1.189	1.183	1.178	1.173	1.168	1.163	1.157	1.152	1.147	1.142	1.137	1.132	1.126
65........	9	5	1.211	5	1.200	5	1.190	5	1.180	4	9	4	9	4	8	3	8	3	8
70........	1.220	6	2	7	1	6	1	6	1	6	1.170	5	1.160	5	1.150	4	9	4	9
75........	1	7	3	8	3	8	2	7	2	7	2	6	1	6	1	6	1.141	5	1.130
80........	3	9	4	9	4	9	4	9	3	8	3	8	3	7	2	7	2	7	1
85........	4	1.220	6	1.210	5	1.200	5	1.190	4	9	4	9	4	9	3	8	3	8	3
90........	5	1	7	2	6	1	6	1	6	1.180	5	1.170	5	1.160	5	9	4	9	4
95........	6	2	8	3	7	2	7	2	7	1	6	1	6	1	6	1.150	5	1.140	5
100........	7	3	9	4	8	3	8	3	8	3	7	2	7	2	7	1	6	1	6
105........	8	4	1.220	5	9	4	9	4	9	4	8	3	8	3	8	2	7	2	7
110........	0	5	1	6	1.210	5	1.200	5	1.190	5	9	4	9	4	9	3	8	3	8
115........	1.231	6	2	7	1	6	1	6	1	5	1.180	5	1.170	5	1.160	4	9	4	9
120........	9	7	3	8	2	7	2	7	2	6	1	6	1	6	1	5	1.150	5	1.140
125........	2	8	4	8	3	8	3	8	3	7	2	7	2	7	1	6	1	6	1
130........	3	9	4	9	4	9	4	9	3	8	3	8	3	7	2	7	2	7	2
135........	4	9	5	1.220	5	1.210	5	9	4	9	4	9	3	8	3	8	3	8	2

Gauge pressure.	125	130	135	140	145	150	155	160	165	170	175	180	185	190	195	200	205	210	212
60........	1.121	1.116	1.111	1.106	1.100	1.095	1.090	1.085	1.079	1.074	1.069	1.064	1.059	1.053	1.048	1.043	1.038	1.032	1.030
65........	2	7	2	7	2	6	1	6	1.081	6	1.070	5	1.060	5	9	4	9	4	2
70........	4	8	3	8	3	8	2	7	2	7	2	6	1	6	1.051	5	1.040	5	3
75........	5	1.120	5	9	4	9	4	8	3	8	3	8	2	7	2	7	1	6	4
80........	6	1	6	1.111	5	1.100	5	1.090	5	9	4	9	4	8	3	8	3	7	5
85........	7	2	7	2	7	1	6	1	6	1.080	5	1.070	5	1.060	4	9	4	9	6
90........	9	3	8	3	8	3	7	2	7	2	6	1	6	1	6	1.050	5	1.040	8
95........	0	4	9	4	9	4	8	3	8	3	7	2	7	2	7	1	6	1	9
100........	1.131	5	1.120	5	1.110	5	9	4	9	4	9	3	8	3	8	2	7	2	1.040
105........	2	6	1	6	1	6	1.100	5	1.090	5	1.080	4	9	4	9	3	8	3	1
110........	3	7	2	7	2	7	1	6	1	6	1	5	1.070	5	1.060	4	9	4	2
115........	4	8	3	8	3	8	2	7	2	7	1	6	1	6	1	5	1.050	5	3
120........	5	9	4	9	4	9	3	8	3	8	2	7	2	7	2	6	1	6	4
125........	5	1.130	5	1.120	5	9	4	9	4	9	3	8	3	8	2	7	2	7	5
130........	6	1	6	1	5	1.110	5	1.100	5	9	4	9	4	8	3	8	3	7	5
135........	7	2	7	2	6	1	6	1	5	1.090	5	1.080	5	9	4	9	4	8	6

NOTE.—The difference in factor of evaporation for each degree of temperature of feed is .00104. Hence for any temperature of feed given in the table, take the factor for the nearest temperature, and add or subtract .001 for each degree of difference.

TABLE IV.

RELATION OF ECONOMY TO EXTENT OF HEATING SURFACE.

(Highest possible evaporation, based upon 24 pounds of air being required to burn each pound of combustible and the temperature of the escaping gases 212°.)

1.	2.	3.	4.	5.	6.
c	E	cE	$E \div 15$	$34.52 \times 1.2 \div E$	$34.52 \div cE$
Combustible consumed per Square Foot of Heating Surface.	Water Evaporated at Atmospheric Pressure from Temperature of 212°.		Ultimate Efficiency.	Coal (with 1-6 Refuse) per horse power per hour.	Heating Surface per horse power.
	Per Pound of Combustible.	Per Square Foot of Heating Surface.		On Basis that one Horse Power requires 30 lb. of Water per Hour Evaporated at 70 lb. Pressure from Temperature of 100°, or 34.52 pounds at Atmospheric Pressure from Temperature of 212°.	
Pounds.	Pounds.	Pounds.		Pounds.	Pounds.
Minimum.	14.20	....	.95	....	....
.1	13.71	1.37	.91	3.02	25.18
.2	13 25	2.65	.88	3.13	13.03
.3	12.82	3.85	.85	3.23	8 98
.4	12.41	4.96	.83	3.34	6.95
.5	12 03	6.02	.80	3.44	5.74
.6	11.68	7.01	.78	3.55	4.92
.7	11.32	7.92	.75	3.66	4.36
.8	11.00	8.80	.73	3.77	3.92
.9	10 69	9.62	.71	3.87	3 59
1.0	10 39	10.39	.69	3.99	3.32
1.5	9.13	13.70	.61	4.54	2 52
2.0	8.11	16.22	.54	5.11	2.13
2.5	7.28	18.20	.49	5 69	1.90
3.0	6 57	19.71	.44	6.30	1.75
3.5	6.00	21.00	.40	6.90	1.64
4.0	5.50	22.00	.37	7.53	1.57
4.5	5.06	22.77	.34	8.19	1.52
5.0	4 68	23.40	.31	8.85	1.48

THE MONONGAHELA SUSPENSION BRIDGE, AT PITTSBURGH, PA.

By Col. Sam'l. M. Wickersham.

Paper read before the Engineers' Society of Western Pennsylvania, May 14, 1883.

In order to a proper conception of construction of thirty-eight years ago, in which time more than a generation of men have come and gone, it might be well to look into the then existing state of affairs bearing on the subject, and try to understand the difficulties which the engineer had to encounter considered in respect to the resources at his command with which to meet them. At that time work in the shop was done principally by hand. The slide rest was a novelty, and the straight edge and steady eye and hand turned out the piston rod. Screws were still cut in the lathe by hand. The whip-saw was used to cut the floor beams of the aqueduct in 1844. The trip-hammer was still doing its noisy work, afterwards so effectually silenced by the squeezer. The canal-boat still controlled the freight traffic between West and East, and steel was almost among the precious metals. But it would occupy too much time to go into this interesting part of the subject. We will therefore ask your kind attention to a simple narrative of events preceding and leading to the construction of the Monongahela Bridge, and to an account of the method pursued in its building. I regret much being disappointed in obtaining some drawings which would have added interest to the description, so must do without them.

Up to the beginning of 1845, the bridges throughout this country were built of wood, wood and iron combined, or of stone, a few of chain—all generally of short spans, wood being the principal material used. The bridges were generally for highways, for foot and wagon traffic. Although about 15 years had passed since the introduction of railroads, they had as yet not been extended very far. It was in 1845 a company was formed to build a railroad from Harrisburg to Pittsburgh. It was in 1845 a company was formed to build a telegraph line from Philadelphia to Pittsburgh. Here commenced the era of railroads which from its necessities produced the present era of iron and steel bridges.

At Philadelphia, Pa., a wire suspension bridge had been erected, spanning the Schuylkill river, at Fairmount, at the expense of the county, by Chas. Ellet, Jr., C. E., who furnished the plan and contracted for the work at $50,000. The abutments and columns were of granite, the distance between abutments 343 feet, between the supporting rollers on top of the columns 357 feet, width of floor and foot-way 27 feet. The wire was laid in 10 cables, 5 on each side of the bridge—each cable extending from anchorage to anchorage over the top of the columns, and were fastened at the ends around numerous stout iron bars transversely imbedded in the solid rock or in an immense body of masonry—this formed the anchorage.

Each cable was composed of 260 No. 10 wires, forming a strand 2⅝ in. in diameter, weighing each 4 tons, and each being 650 ft. in length. They were wrapped at short intervals with bands of wire. The cables laid in a horizontal plane—across the 5 cables iron bars were laid to which were attached the suspending rods, which were composed of small wires aggregating an inch in area, which, hanging vertically, as did also the cables, were attached to the beams that supported the floor. This bridge was opened for travel in the spring of 1842. It was erected on the site of the wooden bridge built in 1818 by Lewis Wernwag, which was well known as being the longest single span wooden bridge in the world—343 feet between the abutments. It had succumbed to fire, the almost universal fate of wooden bridges.

There was no element of stiffness in the Fairmount suspension bridge, and it was subject to great vibrations. It was very graceful and pretty to the eye, but very unsteady to the feet.

This method of construction had been severely criticised by Mr. John A. Roebling, and a controversy had arisen between these distinguished engineers, Mr. Roebling maintaining that suspension bridges could be built possessing elements of stiffness and rigidity by a proper combination of its own elements, that by laying the wires in cables of larger diameters, the stiffness of a solid bar could be obtained. Mr. Ellet held it to be impracticable to combine the wires into a cable of large diameter so that each wire would bear its proper share of the burden, and that therefore the wires must be laid in cables of small diameter, adding to the number as additional strength was requir[illegible]. So the contest rested until 1844, [illegible] Mr. Roebling contracted with the city of Pittsburgh to rebuild the aqueduct across the Allegheny river, connecting the Pennsylvania canal with the basins within the city, which enabled him to introduce the plan of suspension

bridges he had so contended for. There were 7 spans in this work; with 2 continuous cables made of No. 10 wire B. G., each 1175 feet in length from anchorage to anchorage, 7 in. in diameter, formed of 1900 wires each, gathered into a round cable, and tightly and closely wrapped throughout with No. 14 an nealed wire. The cable passed over stone pyramids on each pier and on the abutments, resting there on rollers. The suspension rods passing over the cables descended between the floor beams which were put up in pairs, and block and washer below. The trunk was so made of two courses of $2\frac{1}{2}$ in. plank crossing each other diagonally and held together by the side post and framing, so as to be self-sustaining, so that the cables had really only to carry the weight of the water within it.

The total weight of water in each of the seven spans when the canal was full, was 295 tons; weight of one span, including all, 420 tons; average ultimate strength of each wire, 1100 pounds; tension of one wire, 206 pounds.

Thus for the first time did Mr. Roebling introduce his system. But it was objected that the load in the aqueduct might be considered as constant, and the excess of strength was so great that many inequalities might exist and not become manifest for a long time, owing to the entire absence of vibrations, and therefore it was not a solution of the point at issue.

The work on the aqueduct was drawing to a close; (it was opened for navigation on the 22d of May.) When, on the 10th of April, 1845, the bridge over the Monongahela river, at the foot of Smithfield street, Pittsburgh, which had been erected in 1818 by J. H. Johnson, after designs of Lewis Wernwag, was destroyed in the fire which, on that day, swept over the city, leaving 40 acres of ruins where in the morning had stood the principal portion of Pittsburgh's business houses, the blow was stunning, and for a time it seemed that it would be fatal to our prosperity. But soon the native energy asserted itself and the work of restoration commenced.

The bridge company felt the necessity of re-establishing communication with the South Side, but were in no condition to incur any heavy expense. Every one seemed to be ruined, and it was questionable whether the needed funds could be raised; the cost of erecting the bridge was an important consideration.

Mr. Roebling seized the occasion to make and offer a plan and estimate for a wire suspension bridge, in which the abutments and seven dilapidated piers of the burnt bridge could be utilized, and having all the mechanical appliances required in the execution of the work, together with the skilled and unskilled workmen still with him at the aqueduct, determined not to miss the opportunity of introducing his distinctive method of construction to the world; he therefore made his estimates at a figure just sufficient to cover actual cost, leaving to future works his proper remuneration, and in this the near future richly proved his wisdom. The offer of construction was so low that the bridge company accepted it, and twenty days after the destruction of the old bridge, namely on May 1, work of preparing for the new was commenced.

The abutments and piers of the old bridge had been greatly damaged by fire; the injured portions were taken down, and they were thoroughly grouted before the new masonry was laid.

The piers were 50 ft. in length at bottom, 36 ft. high, 11 ft. wide at top, battering 1 in. to the foot. Two bodies of substantial cut stone masonry, measuring 9 ft. square and 3 ft. high, were erected on each pier at a distance of 18 ft. apart. On these the bed plates were laid down for the support of the cast iron towers, to which the cables were suspended by means of pendulums, each span being supported by two separate cables—there being in the whole bridge 16 cables. Anchor rods for the towers were properly placed and walled in the masonry, anchor pits were dug within the abutments to a proper depth, a plank box made and placed on the bottom filled with cement grout, then the anchor plates with the first links were let down into the cement, a floor of double planking laid on top of the plates and the masonry commenced and carried up, the spaces around the links being grouted with cement as the work progressed. On the curve the knuckles rested on cut stone; thus all the links composing the anchor chains were built solidly in the mass of stone until they reached the surface. Emerging from the masonry they extended a distance of 45 ft. to the top of the towers on the abutment, where they were attached to the pendulums and formed the connection with the cables. They were carried up from the anchor plates on such a line as to throw the strain which they were to resist inside the foot of the abutments, insuring the stability of the structure so far as the ends were concerned.

The towers were composed of four columns connected by four lattice panels secured by screw bolts. The panels up and down stream closed the whole side of the tower, but those in the direction of the bridge formed an open doorway which served for the continuation of sidewalks from one span to another.

On top of the columns a massive casting rested which supported the pendulums to which the cables were attached; the upper pin of the pendulums laid in a seat which was formed by the sides and ribs of a square box occupying the center of the casting. For the purpose of throwing the whole pressure upon the four columns underneath, 12 segments of arches butted against the centre

box and rested with the other end upon the four corners.

The pendulums were composed of four solid bars of 2 ft. 6 in. from centre to centre of pin, 4x1 in., with heads of 8 in. diameter, pin holes 3 in. diameter. To the lower pin the cable of one span was attached directly, and the connection formed with the next cable by means of four links of 3 ft. 6 in. long and 4x1½ in. section. I may here quote the language of Washington A. Roebling, the distinguished son of John A. Roebling, "that the peculiar features of this bridge were the pendulums, as by means of these any concentrated load upon one span was distributed over all the others, from anchorage to anchorage. By means of these it became possible to use the small towers which were built upon the narrow piers of the old bridge.

Whilst adopting the pendulums for this bridge Mr. John A. Roebling did not recommend their general use. In consequence of this pendulum system several times during the existence of the bridge our rivermen were enabled to pass under with their boats, whereas without it they would have had to await the fall of the river—in cases where they lacked up to 9 inches of headway in the channel span—they would have all wagons stopped in the two contiguous spans, thereby depressing them and raising the channel span so as to let them through, and this was often of great importance to them * Before the completion of the piers and abutments an earnest effort was made by Mr. Roebling to be allowed to raise the level of the bridge 10 feet—the expense of which was estimated at $10,000—but without avail; quite a bitter controversy arose on the subject. The up-river interests called for it's raising. Here it was urged that giving a greater headway over the channel might enable boats to ascend direct to Brownsville, making Pittsburgh but a way station between the west and the east. In those days Brownsville was the point where the National Road—one of the main arteries of western travel—struck the western waters—from thence handsome packets brought the traveler to Pittsburgh, by means of the improvement of the river by the Monongahela Navigation Company—and this travel formed an important item in our resources, as the passengers generally laid over at least one night in Pittsburgh before taking steamers for their western homes. This delay furnishing opportunity for our merchants and manufacturers to secure many good customers, and the general feeling was averse to doing anything that might impair this advantage and move the head of navigation to Brownsville. In June 1845 the Brownsville *Herald* charged the Hon. Wm. Wilkins, then president of the Bridge Co., as successfully opposing the rebuilding of the bridge at an increased height on the ground that it would let boats pass up to Brownsville to the injury of Pittsburgh. Mr. Neville B. Craig, then the able editor of the Pittsburgh *Gazette*, in reply said that he doubted the correctness of the report and denied that the rebuilding of the bridge at the old grade or a higher one would have any effect on the business of Pittsburgh, adding—"This is sheer folly. Pittsburgh, from her size and wealth, her geographical position, her situation at the terminus of the Pennsylvania Canal, and as the converging point of roads and trade and means of intercourse with a wide extent of country, is eminently a point for commencing and closing voyages. In this respect no other place on the western waters equal her, except St. Louis and New Orleans. How preposterous then to suppose that the raising or lowering of a bridge is going to affect her trade. We would be glad to see the bridge raised to give our Brownsville neighbors the fullest opportunity of rivaling Pittsburgh." Again on the 26th of June Mr. Craig expressed the wish that the bridge might be raised to the level of Smithfield street. But the reason given for not raising the height of the bridge, and which we may receive as the correct one, was the low condition of the finances, the gloomy existing state of affairs generally, and the fear of getting into trouble by saddling themselves with an additional debt of $10,000, at that time, and all circumstances considered, a fearful amount. The construction of the bridge was therefore continued on the original plan without alteration.

While the work was progressing on the abutments and piers, the wire for the cables was being made. There were at that time but two wire factories existing west of the mountains, "Townsend & Co., at New Brighton, Pa.," which establishment is still in existence and noted for the excellent quality of its product, and the "Pittsburgh Wire Works, Saml. M. Wickersham," which works were swept out of existence by fire in 1850 and never rebuilt. To these two works was given the contract to make the wire for the cables, and about equal quantities were made by each. The greater portion of the iron used in the manufacture of the wire was made and rolled into ¼-inch rods by "Lyon, Shorb & Co., of Sligo Iron Works," and it is well to mention here as showing the advance in 38 years in the manufacture of iron that the rods then furnished generally weighed but 8½ lb., though some were rolled of 11-lb. weight, and when they came from the rolls were so chilled as often to be blue, and always requiring to be annealed before giving them the first break down, and then a second annealing and scouring before finishing to

*Since writing the above, I have been informed that on one occasion, Wm. Robinson, Esq., passed a boat, after—by means of coal wagons—having raised the channel span 14 inches.

No. 10 for the cable. The wire was finished hard and bright. Now it is possible to have rods of steel of 100 lb. weight, and finished so soft as to admit of being drawn into wire of same number with the proper hardness and ductility and strength with but one annealing.

The wire for wrapping the cable was drawn to No. 14 and finished by annealing. The anchor and pendulum bars were made by W. H. Everson, himself, in a small forge consisting of a not very heavy helve hammer and heating furnace, where he could be daily seen with his leathern apron on handling the tongs, 'a prototype of Pat. Lyon, at the very spot on which now stands one of the great iron works of Pittsburgh, and at the head of which Mr. Everson still remains.

The Pennsylvania Iron Works is the 38 years' growth of the modest Pennsylvania Forge.

Mr. Everson made the plates out of Juniata blooms—they were required to be of 65,000 lb. tensile strength per square in.—the fact was they were made of what was considered the best iron to be had, and Mr. Everson's knowledge of the quality was the real test the iron was subjected to. The material used in the structure was closely examined and tested by Mr. Roebling by all the means then at hand—for well he knew that in so light a structure quality became of the greatest importance, and, so far as he could do so, he allowed nothing else to enter into it.

Each wire was tested to 1200 lb. tensile strength, it was also held in a vise or in the plyers and bent to a right angle, then bent over to the same angle in the opposite direction, then straightened up; if it stood this without fracture it was received, if not, it was rejected.

At this time the ground on the South Side above the bridge site was in open fields; there the workshops were erected and the cables made. Two platforms were built of a height equal to the required deflection, at a distance apart equal to the length of the cables; an iron pin fixed on each platform on which a cast iron shoe was placed; a guide wire was stretched from pin to pin, giving the exact length and deflection of the cable. The cables were formed from a continuous wire; the short wires composing it being connected by tapering the ends for about 3 inches with a file, placing the scarfed sides together and wrapping the joint with a fine annealed wire; the wire was then taken from the reel and the bight carried by a wheel from one pin to the other. By means of a hand-windlass and a pair of plyers to clutch it, each wire was drawn up to correspond with the guide wire. When thus 750 wires were in their places they were clamped and wrapped by the wrapping machine, worked by hand, from end to end with the No. 14 annealed wire into a compact round cable. When finished, by the aid of a large screw, the cable was drawn back, slipped from the pins and laid upon the ground; the wires were each coated with boiled linseed oil and the cables well covered with white lead paint to prevent oxydation. When all the cables were completed planks were laid upon the ground and by means of rollers and fall blocks they were moved to the river side and placed on flat boats which had been coupled together end to end, the boats were then dropped to and anchored between the piers.

The towers being well guyed to avoid pulling them over, whilst the cables were being hoisted to their places, blocks were attached to their tops and the cables raised and connected to the pendulums. This part of the work was commenced in the 2d or 3d span from the Pittsburgh side, hanging both cables and working both ways until all were in position. Then commenced the laying of the floor beams; these were of white pine, 31 feet long $4\frac{1}{2}$x15 inches; they were placed in pairs at a distance apart of 4 feet.

The suspension rods made of $1\frac{1}{2}$ in. round charcoal iron were attached to clamps which embraced the cables, and then passed between the floor timbers and through a bearing block and cast iron washer below. In hanging the beams care was taken that the spans should not be loaded too heavily at one place, so when 7 or 8 were hung on one side the span the same number would be put on the other side and thus alternately placing them would finish each span in the center.

The floor was double, the first laid lengthwise of the bridge, the second across; the roadway was 20 feet in width, separated from the sidewalks by fender rails. The sidewalks were each 5 feet in width, elevated a few inches above the roadway, and were outside the cables. The total width between the railings was 32 feet. The railing was an open lattice of sufficient depth to be self-sustaining and was one element in giving stiffness to the bridge. A curious circumstance, and worthy of record, occurred at several times to this railing; it was finished with a broad cap-piece running lengthwise of the bridge, covering and concealing the ends of the plank forming the lattice-work. Owing to the vibrations of the bridge this cap-piece would work somewhat loose and take a sliding motion, and several times the friction thus caused set the rail on fire. The movement was of slight extent, but so rapid as to cause this effect.

The opposite cables, as well as the pendulums, were inclined toward each other, the distance apart being 27 feet at the top of the towers and 22 feet at the center of the spans. The pendulums on the abutment occupied a vertical position.

The floor was further supported by a number of stays, made of $1\frac{1}{4}$ inch round charcoal

iron, extending from the tops of the towers to the beams below for a considerable distance on each side of the piers. Timber supports also extended a short distance from each pier and each abutment; a wooden beam extended across the bridge from top to top of the towers, for the purpose of resisting the side tension of the cables.

As stated, the tearing down the portions of the old abutments and piers, which had been injured by the fire, was commenced on May 1st, 1845. The new work was begun in June, continued without intermission through the summer and fall and following winter—a great portion of the work having to be done in the cold weather of the winter.

The bridge was thrown open to travel in February, 1846, eight months after its building began and nine months after the contract for its erection was signed; but it had been used once before. On the night of 31st December, 1845, the ice in the Monongahela river broke up, owing to a sudden rise. At noon of January 1st, 1846, to relieve the great inconvenience, the first floor having been just laid, the passage of wagons was allowed for one hour, and with great trepidation did the worthy treasurer of the company, Mr. John Thaw, walk to and fro until the whole stream of market wagons and other vehicles, occupying at times the entire length of the bridge, as many as 7 teams being on one span at one time, had passed safely over.

The whole cost of the bridge was $55,000.

The masonry cost	$13,120
The superstructure cost	41,880
In all	$55,000

It is probable that no other bridge in the world, of same length, having a double carriage way and two sidewalks, has ever been constructed so cheaply.

The bridge is memorable as the first example of the solid wire cable, being in great measure depended on to give not only support to the bridge, but also resistance to oscillations. The combination of the wires composing it, into one tightly-bound cylinder, whilst giving a strength of unerring certainty, also gives a stiffness almost equal to that of a solid iron bar, whilst by hanging the cables, with their planes inclining towards each other, a strong resistance is offered to lateral disturbances, and the almost solid inverted arch—which the cables themselves form—with their solid iron rods connecting to the floor offers the same resistance to vertical movements, thus providing against all the forces with which a bridge must battle. This surely entitles Mr. John A. Roebling, its architect and constructor, to a position in the front ranks of civil engineers.

In this bridge many small defects may have been developed in its thirty-seven years' use—some parts may have been made too light—if such have been developed in the taking of it down, it will probably be found that they were such as could have been replaced, as the bridge was so built, that any part of it could have been got at and repaired if injured or renewed if requisite.

The bridge was subject to no greater vibrations than are generally observed in the wooden arch and truss bridges of same span.

It has often been sorely tried, sometimes when crowded with people, viewing a boat race and sudden rushes would be made from one side of the bridge to the other. I have heard from Mr. Roebling that he thought it was tested to half its strength.

This bridge is memorable also as being the first of a series of which the last is the great Brooklyn Bridge, now approaching completion.

In addition to my own personal knowledge relative to the construction of this bridge, I wish to acknowledge my indebtedness for valuable information in the preparation of this paper to:

Washington A. Roebling, of Brooklyn, New York.

Jonathan Rhule, of Phillipsburg, Pa., for long time foreman for Mr. John A. Roebling.

Wm. M. Lyon, Esq., and Wm. H. Everson, of Pittsburgh, Pa.

The editors of the Pittsburgh *Gazette*, who kindly placed their old files at my disposal.

References was also had to:

Frederick Overman's work on "Mechanics."

Neville B. Craig's "Olden Time."

Appleton's Cyclopœdia of Applied Mechanics.

Day's Historical Collections of Pennsylvania.

And Mr. John A. Roebling's own report to the New York *Railroad Journal* in 1846.

TABLE OF DIMENSIONS OF THE MONONGAHELA SUSPENSION BRIDGE.

Length of bridge between abutments	feet	1500
Number of spans		8
Length of each span	feet	188
Deflection of cables	"	14
Number of cables		2
Number of sections of cables		16
Diameter of cables	inches.	$4\frac{1}{2}$
Number of wires in each cable		750
Weight per foot of each cable	pounds	39
Weight of cables and mapping	"	119,000
Length of the 16 cables	feet	3,036
Ultimate strength of the 2 cables		860
Aggregate weight of one span so far as supported by the cable with 100 head of cattle on it	tons	110
Tension of cable produced by dead load of bridge and 100 head of cattle on one span	tons	192
Weight of 100 head of cattle at 800 pounds	"	40
Tension resulting from it when at rest	"	70
Weight of four six-horse teams loaded with 104 bushels of coal each	tons	28
Tension resulting from it when at rest	"	49
Weight of superstructure of one span so far as supported by cables	tons	70
Tension of cables resulting from it	"	122
Section of anchor chains and connecting links	inches	36
Section of pendulums	"	32

IN MEMORIAM.

JAMES PARK, JR.

It is our painful duty to record once more the death of a member of our Society. In this case it is a loss not to us merely, nor to a limited circle of family friends.

The man whose name heads this memorial was known not to us alone, not to friends and acquaintances only, not to the residents of our two cities; his name was national.

Wherever great enterprises flourished, wherever great questions were discussed, whenever great issues were at stake, there James Park, Jr., was sure to be found, great among the greatest, guiding with sound judgment, pushing with great energy, inspiring with his zeal.

Who that remembers the stirring scenes of the great war can ever forget his work; in the darkest hour his faith never faltered, his efforts were not diminished; as weeks rolled on into months, and months into years, and war became a business and sentiment ceased, he kept on in his part of the work with that untiring, persistent, ceaseless energy with which he began. No man in our community was so unsparing of self as he; no amount of work was too much, no hours were too long for him; no matter what the present load might be, he was always ready to take up a little more, and then lend a hand to encourage the laggards. It was the same in business, in public works, in charities and in his church; wherever James Park, Jr.'s., name was to be found, there success was sure to follow. Was cotton raised in the South, and sent in great quantities through Pittsburgh to the East, to be exported, he was among the first to seize it on the way and make Pittsburgh cotton manufactures to flourish. When copper was first discovered in the Lake Superior country, he was among the first to demonstrate that Pittsburgh was the best place to work it, and he helped largely to make the Iron City the great copper city also. When the great Bessemer industry was an uncertain, doubtful, and costly experiment, he was among the few whose courage nursed the strange thing until its magnificent success was assured. When the finer crucible steel manufacture was first thoroughly attempted, when scorn of the bantling was in the hearts of Americans and sneers adorned the faces of our English oppressors, he was among the first to take up the waif, and to pursue its development to the end of his busy life.

And with what result? the Black Diamond Steel Works stand to-day the largest in the world, and with a reputation second to none.

Only those who have stood shoulder to shoulder with him can know what the struggle cost; what care, what worry by night and by day, what weariness of flesh, what wearing of the mind. On one side were want of knowledge, want of experience, lack of skill, and need of material; on the other the skill and experience of a century, with all of the best material of the world at command, aided by the prejudice of the whole host of consumers.

This struggle ended in a brilliant success, and cost Mr. Park the comfort and rest of a serene old age; the effort was too much for even his robust body and iron will; the machine was worn out; he died.

James Park, Jr., was born in Pittsburgh, January 11th, 1820; the site of his native place is now 120 Second avenue.

In 1825 his family moved to Allegheny, and he resided there up to the time of his death, April 21st, 1883.

He commenced business with his father, James Park, in 1837, in the grocery and queensware trade. In 1840, the firm of James Park & Sons was organized, James Park, D. E. Park, and James Park, Jr., constituting the firm. In 1843 James Park died.

D. E. Park, James Park, Jr., and John McCurdy built the Lake Superior Copper Mills, under the firm name of Park, McCurdy & Co. Mr. McCurdy retired in 1863, and the Messrs. Park continued the business under the name of Park & Co. until the death of D. E. Park, when Mr. James B. Scott joined the firm, and the firm was changed to Park, Scott & Co.

The firm of Warner, Park & Co., composed of Judge Warner, James Park, Jr. and D. E. Park, and also the firm of Park, Painter & Co., composed of Jacob Painter and D. E. and James Park, Jr., owned at different times the Banner Cotton Mills, in Allegheny.

In 1870, Park, Painter & Co. sold their mills to the Eagle Cotton Co. In 1862, Sept. 20th, the firm of Smith, Park & Co. was organized, and operated the National Foundry and Pipe Works; the members of the firm were Wm. Smith, D. E. Park and James Park, Jr. On Jan. 2d, 1866, the Messrs. Park withdrew.

The original firm of Park, Bro. & Co., operating the Black Diamond Steel Works, was organized in 1862. In 1875 Messrs. A. D. & D. M. Smith and D. E. Park withdrew, and the existing firm of Park, Bro. & Co., was organized.

Against the above list of great enterprises and others of a lesser nature, not named, there is not a failure to be recorded.

The names and the institutions are synonymous with success, and in the successes James Park, Jr., was a large factor.

What was he to our Society? the same that he was everywhere; the warm friend, the wise counsellor, the earnest supporter. Only two months ago he was in his place here, interested in the proceedings, intelligent in the discussion, and full of that kindly humor which always added a charm to our meetings when he was with us.

What manner of man he? An upright man, as earnest and practical in his Christian faith as in his business life; a warm friend, always ready to lend a hand; an open opponent, respected by all whose aims or views differed from his; a vigorous competitor, but frank and generous, and open in his methods; a strong man, who pursued every object with all his might; an earnest man, who allowed himself too little time for himself; a genial man when occasion permitted, who always stopped pleasure within the limits of moderation; a wrathful man when aroused, who cherished no animosities; a firm man in what he thought to be right or wise; and a tender-hearted, sympathetic man to all honest suffering.

He was a man worthy of imitation; no breath of scandal ever reached him; and in every place of trust or responsibility his name was a rock of safety. Let it be recorded of him then, that strength and purity, honor and truthfulness, faithfulness and honesty, were the foundation stones of his success; they are a noble heritage to his children, and his name is an honor to our community.

Wm. Metcalf,
N. M. McDowell.
Thos. Wickersham,

THE HIGHWAYS OF THE PEOPLE.

By S. B. Fisher.

A paper read before the Engineers' Society of Western Pennsylvania, September 18, 1883.

The great men who chalked out our country seemed to have a huge conception of road interests. In the original grants of land in Pennsylvania six per cent was reserved for road purposes. When the public road system of our country began to grow it branched into three kinds or classes of roads. These were—state, county and township. In addition to these there were the private roads, which, though not forming part of the public roads proper, virtually belonged to the system.

It appears there was a pretty well defined scheme for internal intercourse in the minds of the founders of our government. The great thoroughfares, on which people and traffic were to be thick and frequent, which stretched from county to county, forming the arteries for circulation, were to be under the control of the state. Those in some of the western states in former times were toll-roads. The roads lying within the county and extending through different townships, were to be under the control of the county; while those of mere local importance lying within the townships were to be controlled by township authorities.

The public road scheme was capped by building, during Jefferson's administration, the great National Turnpike from Cumberland to Wheeling; uniting the seaboard with the Ohio valley. This road was to have been continued to the Mississippi river, but never got further than Indianapolis.

The whole system of internal improvements by the general government, canals as well as highways, when it had fairly got under headway, was stopped by the will of the sovereign people. Then sprang up the railway system. Like our present breed of pestiferous rats, which when imported overran the country and exterminated the comparatively harmless native rat, the railroads have squelched the public improvements of the past age. Canals have dried up and are disappearing. The great national highway is kept in miserable repair; formerly a magnificent highway, sixty feet wide, the farmers have now encroached on all its borders.

The triple system of state, county and township roads, while it still exists in law and in name, is practically knocked into a cocked hat and jumbled together into one class of public roads, or lanes for travel, on which people go to and fro, and in which they take no pride and very little interest. The public road system in the United States has actually gone backward during the past three or four decades.

The railroads have done much for the people. They have cheapened travel and transportation to great distances. They have made people travel and transport goods to a degree never dreamed of by the framers of the original highway system. But their benefits to the country at large—like an old friend of mine once remarked about the smartness of certain people of his acquaintance—runs in streaks.

Let us glance at a railroad map. Here runs the railroad through a tier of farms. On these farms it will burn the fences, kill the sheep, and keep the people awake o'nights. Just outside of this tier of farms, for two or three miles on each side, there are people living, who have all the advantages of railroads; who are linked with the whole world and can have untold material comforts. Back of these there is another strip of land, on which the people enjoy these advantages in a lesser degree, and then back of this another, and so on, until the benefits of railroads are not felt. It is probable that the influence of railroads is not directly felt much beyond a distance of ten miles on either side. Not only this, but their benefits are dispensed only along this strip of country at those points called *stations*. These stations are often not very frequent at the present time, and the tendency on leading railroad lines, with heavy traffic, seems to be to make them still further apart. It is very probably that distances of from six to ten miles between regular stations would contribute very considerably both to economy in the equipment and to economy and safety in the operation of railroads. On leading railway lines it is difficult to get new connections for local traffic. Thus the influence of railroads on the country is confined to *neuclei*, of which the railway station is the centre. In fact the railway system has taken up *part* of the primitive public road scheme, that part which was assigned to the national, state and partly to the

county roads, or transportation to great distances, and has carried it far beyond the dreams of the originators. It has left the local transportation question untouched, except incidentally.

If a system of roadways for local travel and traffic is ever carried out, it is natural to suppose, that it will be grafted on to the railway system, that it will radiate from the railway station.

The importance of the local traffic has perhaps in our day been overlooked. The quantities of material transported by local means, never has been and never can be accurately computed. A large part of the time and energy of the entire rural population, all over the country, is spent in local transportation of person and effects. I think we would be safe in saying, that one-third of the time, strength and energy of the people of the country who labor, is spent in local circulation and hauling. The aggregate of it is enormous.

In towns and villages which draw their supplies entirely from the surrounding country depending on public roads, a great fluctuation in the price of these supplies is produced solely by the difficulty and cost of hauling. The whole system of the people's roads become almost impassible during portions of autumn, winter and spring, just at the times when most needed, when the soil being saturated with water and so unfit for tillage the farmers on account of their enforced leisure, could and would use them to advantage.

In short, the necessity of a vast improvement of our public road system is seen from the great amount of travel and traffic on them—from the time spent and energy lost in using them—from their failing when most needed—from the increasing demands of commerce and civilization, and from the failure of railroads to take up local transportation at all comprehensively, or even incidentally to the satisfaction of the people.

A theoretically perfect, or if you please utopian system of public roads, should be free to everybody to use during every hour of the day, and alike defying rain and frost, it should preserve its characteristics as a highway system capable of carrying traffic at all times and in all seasons.

I know not how the roads the old Romans built would be adapted to modern traffic. The old national turnpike was perhaps the nearest approach to a perfect road Americans have ever seen. A comparatively light gradient was first made and on this was built a Telford or McAdamised road with lime stone. Let us suppose for a moment, that we have not only one such road as the national turnpike, but a net work of them, directly uniting county seats and other considerable towns, numerous villages along them and bordered everywhere by fertile farms. The immediate result of such a system would be to give better facilities for the existing traffic, to increase the value of the land embraced in the system and the commercial value of the time and labor of the people living on it. But a far greater result, we think, lies in the prospective development.

Motor for traversing such a road, capable of a moderate speed, say six to eight, or even ten miles per hour, and of hauling a load of several tons, have long ago been built. With a demand for them, they will be built to greater perfection. Steam traction engines for threshers are now actually used as a feature in agriculture. (The bicycle is only awaiting such roads to overrun the country). Such roads and motors would practically solve the local transportation problem for small manufactories. Factories could thus be built any place on the line of a turnpike and connections made with one or more railways at the nearest or most accessible stations. An almost unbounded field would thus be opened up for starting and carrying on small manufacturing interests. These interests started draw others in their train. The effect of such roads would probably be, to join together—to fuse more perfectly the ordinary operations of mining, agriculture and manufacturing as carried on by the people. And the system would be especially adapted to a country with varied products, and everywhere, where local travel and traffic are or will be great. On such roads the capacity of the ordinary team of horses and mules is increased some two or three fold.

The great advantages and desirability of such a system of roads are evident and admitted by nearly all. The great obstacle is in their cost. In those localities where the local highway has been improved, in the streets of large towns and cities, the cost of paving is very great, and in suburban districts has sometimes swamped the property. The great barrier to highway improvement is the cost of it.

The national highway from Cumberland to Wheeling, 130 miles in length, cost $1,700,000, or $13,000 per mile. This includes stone arch bridges with elaborate approaches contained in long retaining walls, grading and crossing the Alleghenies, as well as the construction of the permanent way. In improving existing highways a very limited amount of grading only would be required, and would generally be confined to reducing steep gradients, usually for short distances. The proper drainage and preparation of roadbed would require considerable work. The stone required, if the road is built of stone, would be the largest item. A roadway 24 feet wide and 18 inches deep would require 7000 cubic yards of stone per mile. This in a country where it abounds might be obtained for say $7000. Bridges would require strengthening. The value of such improvement could thus be taken at say $10,000 per mile. If the ad-

joining land was assessed to make such improvement, say for a mile on each side, or 1280 acres at $8.00 per acre, the assessment would be $10,240. The commercial value of a farm lying on such a road as we have supposed, over one lying on the ordinary highway, would be probably from $10 to $15 per acre.

One way which has been proposed for making public road improvements is for the county to take hold of it and levy a light general tax. To proceed with road improvements gradually. To consider the various most important thoroughfares first. To take bids, or rather bonuses from people living on, owning property on or interested in them, and to first proceed with that particular road on which the bonuses are largest.

This scheme contemplates a highway growth, rather than a spurt. If we could get highways to grow, starting from the railway station and from the town, to reach out a little, be it ever so little, year by year and go on until they would reach each other, and then branch out in other directions, slowly yet continuously, we might have great hopes of our local transportation problem solving itself. But if ever the American people undertake to better the local transportation system we may expect them to go at it with a rush. It will probably take hold some of these times like the spelling mania did a few years ago, or like the base ball furor at present, and be run until it is literally driven into the ground. Arguing from all analogy—from the pulsations of commerce—from the fluctuations of business from prosperity to panic, we may expect highway improvement to come in waves. Like it has been in railroad building, a period of great activity succeeded by torpor continually, we may expect the people to run over head and ears some of these days into highway improvement, and then to crawl out of them with popular denunciations, only to pitch in again with renewed vigor after a rest. Probably a large plurality of persons to whom a public road improvement scheme would be presented, at first sight would pronounce the whole thing preposterous, arguing from the expense in paving streets that no county could afford to make such improvements.

If a prophet 50 years ago had revealed the present immensity of railroad interests, to the then people, he would have been considered a hopeless crank. Millions on millions of dollars have been put into railroads and the country has gone on building up and growing in every direction. The railroads have returned more than they got. The greatness of our country to-day, as every one says, being largely due to railroads and railroad influence. If, then, the country has increased in wealth by the development of a lop-sided system of internal intercourse, with such immense outlay for it, why should it impoverish it to complete the system by filling it out and rounding it off?

The local transportation question has become more and more important with each successive revival of trade and is rapidly becoming a problem, which like Banquo's ghost, will not down. That the local transportation question will come sometime in vast proportions is inevitable; that it will come soon, is probable. When it does come, it will have especial interest to us as engineers, for we will be called upon to direct it. Whether it moves along the old established lines of making McAdamized and Telford roads, or along experimental lines and in new directions, it will alike call for skilled direction.

The object of this paper is to call the attention of the society to this subject, and to open up before the members of the profession the neglected field which is their legitimate possession.

FOUNDATIONS FOR RIVER BRIDGE PIERS.

BY P. F. BRENDLINGER, C. E.

[A paper read before the Engineers' Society of Western Pennsylvania, Oct. 16, 1883.]

The subject of this article is indeed an important one, and one of the greatest in the study and practice of engineering science. The title of it is, in a certain sense, rather misleading, for it is not intended by the author to be a treatise in general, on foundations for river bridge piers, but a history of the author's experience in this line, in the vicinity of Pittsburgh. This Society is composed of members who are thoroughly posted in the geography of Pittsburgh and vicinity, so when a certain point or place is mentioned on the Ohio, Monongahela & Youghiogheny Rivers, everyone, no doubt, knows just where that place is.

I will try to be as brief in my article as possible. The first bridge I had charge of as resident engineer in this vicinity was the Port Perry Bridge on the Pennsylvania Railroad Branch to connect with the Pittsburgh, Virginia & Charleston Railway. This bridge crosses the Monongahela River at Port Perry, twelve miles south of Pittsburgh. In August, 1872, work was commenced on this bridge. It turned out to be the most difficult bridge to build that I have had in my experience to date—there were nine (9) piers in the river, the spans varying from 145 to 265 feet,—the river at this point is wider than at any other point in its entire length, caused by the washouts on both sides by the eddies produced by the dam which crosses the river four hundred (400) feet above the location of the bridge. The season best suited for putting in the foundations for piers is unquestionably the fall of the year, the water is then low, and work can be done to better advantage. The foundations can be put in and work continued in the spring. The first step taken was to ascertain from the oldest resident in the neighborhood the lowest stage of water attained by the river, and also by actual observations with the level, then a longitudinal profile obtained on the centre line of the bridge location across the river. The piers were then located on this profile, and by means of iron

rods one inch in diameter, and varying in length according to the depth of water, soundings were obtained of the nature of the material. The bottom of the river varied from one foot to seven feet below low water. The plan followed for the foundations of the piers was to build hollow cribs as shown in plan No. 1. The cribs were built of sound hemlock timber; this timber was chosen because it was just as good for the purpose as oak, pine, or any other timber; hemlock out of water, and exposed to the changes of weather, is of no use at all in railroad building, but under water will last as long as any other kind, and as cheapness is of immense importance, it ranks as the most suitable lumber in the market.

The first course of timber was rafted in the water in pieces of one length, placed at right angles to the long axis of the pier; on top of this was thoroughly spiked a three inch course of pine plank, laid lengthwise on the crib; then six pieces, 12 × 12, as shown in plan were laid lengthwise, properly spaced and the cross pieces framed into the longitudinal pieces, by halving the cross pieces and longitudinal pieces, and dovetailing the two outside longitudinal pieces.

The longitudinal or string pieces and cross pieces thus framed, formed one thickness or course of twelve inches. This course was then thoroughly bolted with rag bolts 18 inches long and 1 inch square, passing through the top course and into the lower course 6 inches; this process was continned till the requisite height was obtained for the crib. The top of the crib was calculated to be just two feet below extreme low water. The crib was never finished until the foundation was ready to receive it, so that the exact number of courses could be built. The crib proper consists of one solid bottom course of timber, and a number of framed courses with pockets scattered throughout. The solid course at the top with the three-inch deck belong to the caisson, and is not attached to the crib until the latter is in its proper place. The caisson is built in this manner; a raft of timber of uniform length of 16 feet was entirely covered by close joints with 3-inch pine plank thoroughly spiked to the same, then side vertical studding six inches by three, mortised and tenoned into the floor, and spaced three feet apart. These studdings were from 6 to 8 feet long—depending on the stage of the water—two inch plank were securely spiked to the studding, thus forming a floating box similar to a coal boat. The flooring and sides were then thoroughly caulked with oakum. Having now described the crib and caisson, I will describe the method of preparing the foundations. By numerous researches and examinations, it was discovered that rock could not be found at a less depth than forty feet, so it was determined to build on the best material that could be found. The top material at the bottom of the river was composed of mud and debris of various kinds. It was immaterial to the railroad company whether the contractor would build a coffer-dam or use a dredge. Dredging was resorted to, and as there was but one dredging concern about Pittsburgh, viz: the Monongahela Dredge Company, there was no chance for receiving competitive bids. As soon as the positions of the piers were marked in the river, the dredge got at its work. As the contractor was paid for the material excavated, it was necessary to take cross-sections of the bottom of the river, both before and after the excavation was made, and also to have an assistant on the boat to prevent

the dredge from excavating the whole river, for they will do it, especially when the material and prices are good. Having reached a clean gravel bottom, the assistant on the boat places the scoop of the dredge at that point as near vertical as he can, then makes a mark on the handle of the scoop or bucket and the dredge proceeds to excavate the whole foundation to that depth. The assistant constantly watching the stage of the water, on a water gauge he has previously established, and as the water raises he raises his mark on the bucket handle, and vice versa. It is essential to excavate the foundations from 5 to 10 feet wider than the crib to insure full width for the crib when sunk. I had the pleasure of excavating on one side of crib No. 8, after it was sunk, because no margin was permitted before this.

Having sounded sufficiently to determine that the bottom was level or nearly so, the dredge boat was moved to another pier, and the process was repeated.

The crib was then towed to position and thoroughly anchored by four ship anchors, placed about 100 feet diagonally away from the corner of each crib, and connection made between the anchors and crib by 1½-inch lines. A boat loaded with rip-rap stone was then moored on each side of the crib, and the pockets of the cribs filled with these stones, and thus the crib was sunk to the bottom of the river. The crib was then well rip-rapped on all sides. As soon as the crib was down, divers cleaned off the tops of the cribs of all stones lying there, and taking great care to level off the top of the pockets, so that no stones were higher than the top of the timber. This is of immense importance as will be shown further on. Having cleaned off the crib, the caisson is floated out over the crib. This needs no anchors, as it is lashed to two end studdings secured to the outside of the crib before the crib is sunk. The caisson is then sunk to its place on top of the crib by building masonry inside in its proper place, as indicated by points marked by the engineer. Pumps are required in the caisson, for despite the care taken in caulking there will be leaks. As soon as the caisson is down solid, levels are taken on all corners of the masonry, and if only a few inches out of level, the top course of masonry is levelled down; if the variation is considerable the level is obtained gradually in several courses, this can be attained by cutting down a course after it is built or setting deeper stones at the low points thus saving considerable cutting. While sinking the caisson, the water pressure is quite severe on the sides, which bend, and leaks are sprung. To prevent this the sides are well braced against each course of masonry as it is set. This avoids intermediate cross-bracing of the sides between the top and bottom. After the caisson is down, it is only a mechanical job to build the pier, for as the late Charles Collins, Chief Engineer of the Lake Shore Railroad often remarked, "Anybody can build a pier, but it requires brains to build a foundation."

The plan of crib shown in plate No. 1, and method here described, was followed in building the foundations for piers 8 and 9, in the fall of 1872. Work was at a standstill during the winter, except in building the cribs for the seven remaining piers, which were made 50 × 12 feet, instead of 61½ × 16½ feet, being smaller piers. There were four framed courses put together, having *no solid* bottom course. The first course instead of being

solid, was framed with pockets like the rest, and the alternate row of pockets were planked over and the intermediate pockets left open. The top of the planks were brought flush with the top of the first course by laying them on cleats spiked to the inside edges of the course, the planks were then spiked to the cleats.

This plan was adopted to save timber, and to allow any loose material at the bottom of the foundation to squeeze up into the vacant pockets when sinking the cribs. The dredge could not remove all the ridges and inequalities of the ground when excavating, and this method of leaving alternate pockets open, was thought, would assist in bringing the crib level and give uniform bearing. When the spring of the year arrived, dredging was resumed, and the freshets were so numerous that frequently when the foundation of a pier was finished, before the crib could be sunk, the hole was filled again by a freshet. This was vexatious to say the least, and anxious were the faces of all concerned for several months. The first foundation started in the spring was pier No. 2.

The material excavated was first a layer of mud two feet deep, then a layer of sand a foot deep, then mud a foot, then sand, and so on alternately mud and sand, and finally sand and blue clay. This was perplexing, as there was no telling when a good bottom would be reached; in the mean time freshets would fill in one night what was taken out before, this caused considerable blanking. By this time the expense of dredging was an immense item to the contractor, and by the permission of the chief engineer, the dredge was allowed to depart, and another—a sand dredge—engaged. After this Lilliputian worked a month, and doing more harm than good, the old dredge was brought back, and by a favorable change of the weather, dredged down to a point 17 feet below low water, when a clean bed of sand and gravel was reached. But below this was another bed of blue clay, which it was decided not to disturb, but build on the sand and gravel. The crib for this pier was being pushed forward rapidly, while the dredging continued, so when the right depth was reached, a crib, composed of fourteen courses was necessary. This was placed in position as usual, and the sinking by means of rip-rap commenced. Care had to be taken in sinking this crib, that no stones were thrown in the open pockets which were marked by vertical strips of boards nailed to them. Everything went lovely, and when the top of the crib was only above the water, about one foot, I was seated in the edge of a rip-rap boat with my feet cocked on the edge of the crib and arms akimbo, and without a moments warning, the crib shot out of the water six feet, and I was landed heels over head in the bottom of the boat, "I told you so," remarked the contractor, swearing profusely, "what did you tell me," I remarked, "why, that those cleats would let the bottoms drop out;" I told him he was wrong, and by a sounding hook convinced everybody that the two lower courses of the crib had pulled out, in other words there were not enough rag-bolts to hold the courses together. For when the crib was started, it was calculated that six courses would be sufficient, and the number of rag-bolts were placed accordingly, but eight courses had been added to this crib, and consequently the buoyancy of the crib was immense, and the weight of the stone pressing down the bottom courses acting against the buoyancy of the thirteen courses above, caused a big strain, and the lower

courses being thoroughly water-soaked by laying in the water all winter, the surface round the rag-bolts became lubricated and the bolts let go easily. Well, here was a nice predicament, two courses of timber with a pile of stones lying at the bottom of the foundation, and a twelve course crib without a bottom to any pocket, floating about over the top. Something had to be done, and that very quickly. I immediately sent for enough 3-inch pine plank to cover the whole top of the crib, then built one solid course of timber on top, and put enough rag-bolts in, to hold it well and strong to the course below. I got eight planks 3″ × 12″ by 12 feet long, and by means of the dredge boat, raised one side of the crib, and and spiked four planks along the side of the crib vertically. I then turned up the other side of the crib and spiked on the other four planks, then as the crib was nearly square, I got the bucket of the dredge underneath the lower edge of the crib and turned the crib completely over, and what was formerly the top was now the bottom; the dredge pulled the crib a short distance away from the foundation, then cleaned out the debris from the foundation. In the mean time another course was built on the crib, and the four planks were spiked on each side, at the top, and the crib was sunk in place, in two days after the mishap occurred. The side pieces, above mentioned were spiked on to hold the crib together.

The rest of the cribs were only built six courses high, and as the material found at that depth was just as good as at twenty feet, no mishap occurred. Having now described these cribs and foundations, I will proceed to give my opinion on them.

I never approved of the plans of the cribs nor of the method of sinking them; in my opinion they are bad. Of course in my position I had to carry out the orders and plans of my superior officers. An engineer in this respect is like a soldier, he must obey orders, besides I was a youngster and had perhaps a younster's ideas, but the plan of cribs I would have desired to build then, I have built since, and the older I get the more I am convinced I was right then.

In the first place the Port Perry cribs were too small, being only six inches wider than the masonry, and were built plumb from top to bottom. This was too small a margin to allow for shifting the masonry in case of not getting the crib in exact positions, for the crib is very liable to surge back and fro out of position, when a steamboat passes.

Second, the crib being sunk separately from the caisson, the stones which are to be thrown into the pockets, are liable to be thrown on the outside and fall under the edge of the crib, causing the crib to be cocked up on one side; besides when the crib is down it is a very difficult task to free the top of the crib of all stones, especially when the water is six feet above the top of the crib.

Of course, you may say, employ regular divers in their armors to go down to clean off the top. This would be an easy matter, but divers are very expensive and contractors like to avoid their employment. The plan of cribs for the small piers were much more objectionable than that for piers 8 and 9. These cribs had in addition to the above objections, alternate open pockets; no matter how much care was taken, there was still very great danger of throwing stones through these openings down under the crib, but whatever stones were thrown in there by accident in these

cribs, were squeezed into the stirred up bottom by the crib, and caused no unevenness in the cribs. The piers were all built up well, and to this day no signs of settlement or cracking of stones are visible.

Before quitting the Port Perry bridge, I wish to give my ideas of this dredging monopoly, for it certainly was then and has been up to a late day. Of course the best way always is to make a contract per cubic yard excavated, and unless a very tight contract is drawn, the dredge parties will soon quit you, especially if the material is hard, and the bottom is required to be closely leveled off, and to get a tight contract with them is almost an impossibility, for they will say, "if you don't like my terms you can go somewhere else." Of course this you can't do, but for goodness sake do not engage them by the day, for if you do they will idle away more time than they work, for the man in charge of the boat, will continually "bob up serenely" with "there is something wrong with the boiler" or "there is a link in a chain almost broken," or the attending steam tug has gone aground somewhere and won't come to you to take away the loaded scow. There is always something wrong, and you might just as well hurry a balky mule as hurry a dredge boat working by the day, and the older a dredge boat gets the less work you can get out of it. I mentioned in the beginning of this article that the amount of material excavated in the foundations was measured by cross-sectioning the bottom before and afterwards. This is a very poor way, and does not do justice to the contracting parties, for frequently when the bottom is excavated it fills again and again, and engineers cannot be about all the time taking cross-sections, and besides it takes time and the dredge boat won't wait. The only true way is to measure everything in the scow, this is divided into compartments, and calculating the contents for every foot and fractions of a foot in height, by measuring the compartments, and the assistant on the boat, by the aid of a graduated rod and table, can ascertain the quantities in a scow as fast as the material is dredged.

The entire masonry was completed in August, 1874, just two years after the work was commenced. This is much longer than should have been the case, but the delay was caused by the panic in 1873, when the monthly estimates were cut down to $2,000. I will also state that this was the last place where I used rag-bolts for the cribs. I found that smooth, round bolts were much better, as they did not cut the fibres of the timber, and held much stronger.

THE POINT BRIDGE.

The next bridge I had charge of as resident engineer, was the Point bridge, the location of which every one here knows is at the mouth of the Monongahela River. This bridge, as far as the masonry is concerned, was a much simpler bridge to build having two abutments, both on land, and two piers, both of them within 150 feet from shore. The greatest annoyance here was the continual passage of boats, and the crowding in of coal boats or barges.

The cribs were built here more according to my ideas than those at Port Perry, still they did not entirely suit me. I have shown in plan No. 2, the plan of cribs for the Monongahela River Bridge on the P. McK. & Y

R. R., which with the exception of the batter to the cribs, was the plan used for the Point bridge piers. The cribs for the Point bridge piers were 90 feet long, 34 feet wide, and 10 feet high. They were built solid from bottom to top, and each course was bolted to the next course below. When I say the crib was built solid I do not mean altogether solid with timber, for after the first course was put together, the next course had every third stick of timber left out, and the space filled with broken stone to keep the crib down low in the water. Every course afterwards was thus filled in with broken stone so that when the crib was at its proper height the top course only partially projected out of the water. Otherwise owing to the buoyancy of the timber, about one-third of the entire crib would have been out of water, and would have taken more masonry to sink it, after the crib was finished, the top course being a solid layer of timber, with 3-inch deck. The sides of the crib were boarded up similar to the Port Perry cribs and caulked, and the crib with caissons attached, was anchored in position over the foundation of the pier previously prepared, as at the Port Perry bridge, by the same dredge boat. The masonry was then built in the caisson, and by this means there was but one sinking necessary. The entire work of sinking was done in two days. In this crib I had allowed two feet margin on each side, for the purpose of shifting the masonry in case the crib would not go down at its proper place.

The contract for this bridge was signed before I was appointed resident engineer, and the specifications called for the masonry to start on a timber platform at the *plane* of low water. This was a stunner to me, I had never heard of such a clause or requirement in any specifications before, and the mere idea of bringing the timber to low water, was such a novelty to me, that it almost took my breath. The minimum allowable distance below low water, has always been to my knowledge, one course of masonry, or say two feet. At my persistent efforts this was obtained in the Point bridge cribs, and it was a lucky thing it was done, for after the bridge was built, the water at the Market street gauge, which was taken as the datum for the Point bridge, lowered over a foot below zero, and the timbers would have been exposed half the season of the year. The abutments were excavated on dry ground, and putting in the foundations was only an ordinary occurrence.

OHIO RIVER BRIDGE, P. & L. E. R. R.

The next bridge I had charge of as resident engineer, was the Ohio River bridge, on the Pittsburgh & Lake Erie Railroad, crossing the Ohio River at Beaver. This was another of those cases where a resident engineer is a soldier and obeys orders without questioning anything. My position here was peculiar. The contractor employed the engineers, paid them, and of course whatever criticisms the engineer made, was done in the face of the man who paid him, but contrary to the usual way, the contractor, Mr. B. J. McGram, of Lancaster, Pa., would not allow any bad work to go in this bridge, and my associations with him were very pleasant, and always agreeable. The only objections I had to the work was the plan of cribs. Here, as in Port Perry, the cribs were built in pockets and sunk separately from the caisson. There were four piers in the river. No. 1,

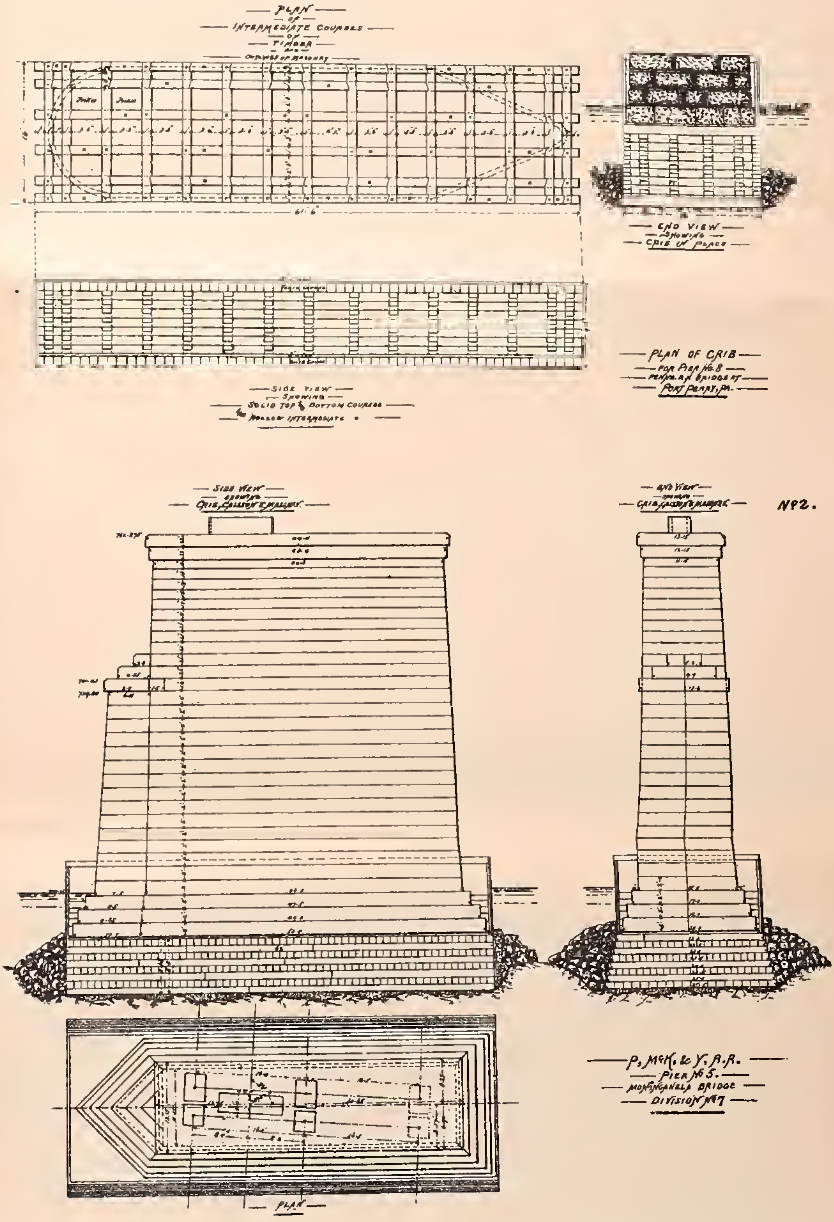
next to the Phillipsburgh shore was built on solid rock, about two feet below low water. No. 2 was built on solid timber foundations, laid on a gravel foundation. A coffer dam was built round the space occupied by the foundation, and it took six weeks to put in this foundation at a cost of over $3,000 to the contractor. It required three pumps, going all the time to keep the water out, two Blake pumps throwing 12-inch streams and one centrifugal pump throwing an 8-inch stream,—the latter doing more work than the two other combined. The coffer-dam was three times abandoned, owing to raises in the river, and it was only by pluck, grit, and perseverance, that the foundations were finally gotten in. This convinced me that coffer-dams are a luxury that should be dispensed with in rivers where dredging can be done. The foundations for piers Nos. 3 and 4, were excavated by dredging. No. 3 was placed nicely and without any trouble, but No. 4 was where I got disgusted with pocket cribs. The foundation was prepared by dredging the crib, being similar in principle to the Port Perry cribs. It was sunk nicely, and the top was cleared of all stone, as was supposed, and the top of the crib was very little out of level. The caisson was sunk the usual way, but when down the masonry was considerably out of level; by soundings it was ascertained that there was an open space between the top of the crib and bottom of the caisson, at the upper end and left side. There could be no question as to the cause of this, for undoubtedly there was a stone or more than one on top of the crib. It was decided to keep on building, as the timber was soft it was thought the stone would sink into the timber by the weight on top, but after the pier was built 43 feet high, the railroad company engaged a diver who examined the foundation and reported a space between the caisson and crib big enough to stick a leg in. The contractor secured a diver and sounded: he reported a small space only at one place large enough to put a hand in flatwise. This contradictory statement, I presume, was caused by the fact that each man worked for the party who paid him. However, the pier was torn down, the caisson removed, and the crib cleaned off good, and a new caisson with wedge shaped bottom timbers sunk in its place, and the pier was rebuilt in eight days.

BRIDGES ON THE P. McK. & Y. R. R.

In July, 1881, I received the appointment of Principal Assistant Engineer of the Pittsburgh, McKeesport & Youghiogheny Railroad, with headquarters here in the city. This road extends from the Pittsburgh & Lake Erie Railroad at 21st street on the south side to New Haven, opposite Connellsville. The route is up the left bank of the Monongahela River to City Farm, where the Monongahela River is crossed by an iron bridge and viaduct 5,400 feet long; thence up the right bank of the Monongahela River, crossing the Youghiogheny River at McKeesport, thence up the left bank of the Youghiogheny River to New Haven, opposite Connellsville.

Among my duties on this road were designing the plans for bridges and foundations for the same.

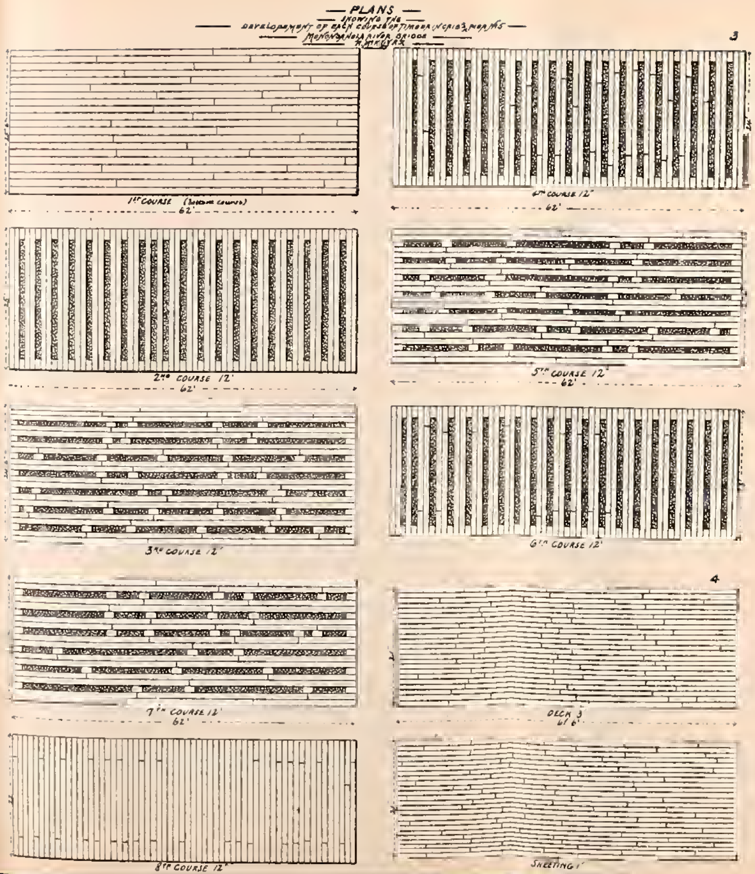
The Chief Engineer of this road, Mr. J. Wainwright, and myself were college chums and classmates, and our engineering experiences were familiar to both, and the confidences and trust reposed in each other was

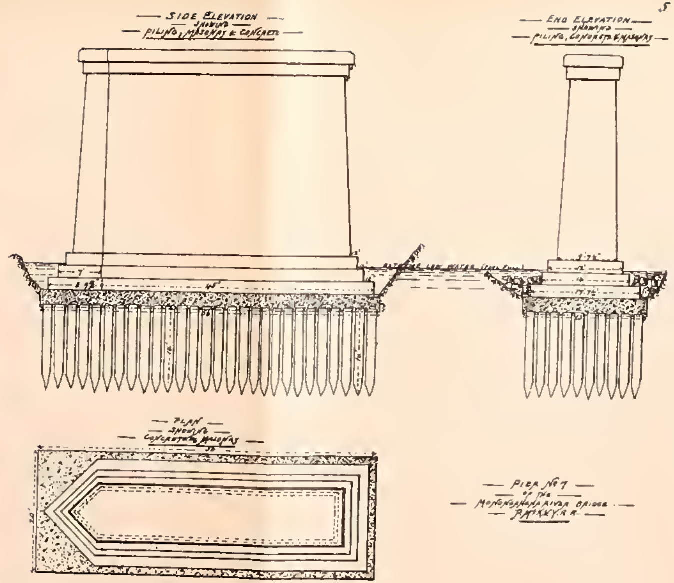


ENGINEERS' SOCIETY OF WESTERN PENNSYLVANIA.

FOUNDATIONS FOR RIVER BRIDGE PIERS.

[To Accompany Paper by P. F. Brendlinger, C. E.]





ENGINEERS' SOCIETY OF WESTERN PENNSYLVANIA.

FOUNDATIONS FOR RIVER BRIDGE PIERS.

[TO ACCOMPANY PAPER BY P. F. BRENDLINGER, C. E.]

unbounded, consequently there was a mutual understanding, and as both of us recognized the great importance of stable foundations in all structures, there were no pains or expense spared to secure the very best work, and I can safely state here that there is no road in the United States that can show as good and secure foundations for all masonry structures in water or on the shore, equal to those on the P. McK. & Y. R. R.

YOUGHIOGHENY BRIDGE.

The first bridge started was the "Yough" bridge at McKeesport. The plan of the cribs for all these piers is the same as shown in plan No. 2, here shown for pier No. 5, of the Monongahela River bridge, of course the sizes varied according to the depth of water and size of pier. The piers at the bottom had generally three projecting base corners, each 2 feet thick, and projecting about one foot on each side over the course above, then the top of the crib projected one foot all round beyond the lowest base course and battered on the side 3 inches per foot to the bottom; the courses were built solid from bottom to top, each course being 6 inches narrower than the one immediately below. The top course was covered with 3-inch pine plank laid with caulking joints and tightly caulked, then a one-inch course of pine sheeting was laid on the decking, covering all the joints below, and it was also caulked. This top sheeting was put on to keep the oakum in the decking, for the pressure of the water below would have a tendency to drive it out. The sides of the crib were then boarded up, having a strong line of vertical studding from 12 to 15 feet high and well cross-braced. This high caisson was necessary on account of the depth to which the crib was sunk below low water. The top of the cribs for the Youghiogheny Bridge were all sunk over 8 feet below extreme low water or pool full. This was done to be on the safe side in case the dam at Port Perry would break and drain the pool. The Youghiogheny River bottom at McKeesport has from three to four feet of a mud deposit, and beneath this is a gravel bottom. The water varies in depth from seven to twelve feet below low water.

The crib for pier No. 3, was the first one sunk; work was commenced on this in the fall of 1881; dredging was resorted to as in the Port Perry bridge. At a depth of about sixteen feet below low water a very good bottom was reached, but by sounding, several large boulders were encountered. These were with great difficulty removed with the dredge, and by doing so the bottom was put in a very bad shape. The bottom was then leveled off to the bottom of the pit where the rocks were. But before this was reached, another boulder was struck, which in turn was taken out, and more boulders found. This was very perplexing, and the question came up:—what was to be done? It would not do to put the crib down on this bottom, for there would be great danger of it breaking in two. A coffer-dam suggested itself:—this would be very expensive; piling was next thought of:—this was a doubtful resource, as if the rocks were numerous, piles could not be driven, besides the crib was built which would be useless if either piling or a coffer-dam be used; it was finally resolved to take broken stone, the size of broken ballast on railroads, and level off the bottom of the foundation to a height of 6 inches above the highest peak of rocks in the bottom.

This was carefully done. The crib was then placed in position and anchored; it took three courses of masonry to sink it in place. When two courses were in, the crib was placed exactly in position and the anchor lines made very taut. The flood-gates were then opened at both ends and the crib sunk nicely in position. These flood-gates are very important adjuncts to a crib. They are placed on the ends of the caisson and made about a foot square, having a rubber seat or bearing for the valve. A vertical stem extends to the top of the caisson, from which place it is raised and lowered. As soon as the crib was down solid, the stones for the next course were piled from end to end through the center of the pier, and when enough were in, the water was pumped out to the top of the highest course set, then the stones were lifted one at a time and set to their proper places. This sinking of the crib by means of water is a good practice, for when the crib is sunk to a great depth it becomes an ugly monster to handle, the waves of passing boats constantly shove it to one side.

All the Yough piers were sunk like this, i. e., there were no pretensions made to get a level bottom, just so a good clean gravel was obtained, and the highest ridge in the foundation was taken as the right height and the whole foundation leveled off to that height with small stone.

THE MONONGAHELA RIVER BRIDGE.

At the Monongahela River bridge the best and cheapest results were attained. An accurate profile was made of the river in the center line of the bridge, and the piers located. At every pier, soundings were carefully taken, and at no place were there more than 2 feet of soft material found, and at every place a very hard gravel bottom was found underneath the soft deposit. The water was at several places sixteen feet in depth. The contractors made a close contract with the dredge company, and the entire dredging for the five piers in the water did not cost over $1,200, while at McKeesport the 4 foundations cost over $6,000. At the Monongahela River bridge the position of each pier was indicated by four line piles, one pile about 100 feet above the pier in the direction of the center line, and three driven in a triangular group about 20 feet above the pier. The two outside piles in this group were in line with the outside of the crib, and the other pile in line with the center line. These three piles were well braced and used for tying boats, etc., to. The upper pile had a cross-plank spiked on, and the outside line of crib marked on. The dredge boat then dredged the foundation for that pier by these lines and distances, and without fooling away a great deal of time and money trying to get a level bottom, the dredge was pulled out, as soon as the soft material was removed, and put at the next pier, and a large load of broken stone was put in place, and soon the foundation was leveled off as even as a floor; then three piles were driven, on each side of the foundation in a line parallel with the crib when in place, and about three feet outside of the crib. The crib was then shoved in at the lower opening and thoroughly lashed to the piles as secure as could be made; there was no slipping of anchors or breaking of lines in this case, but like a squatter, "she just sot thar," and the sinking was done in the usual way. Plate No. 5, shows another kind of foundation. This is the foundation for pier 7, Monongahela River bridge;

it is the shore pier on the south side. The foundation was excavated by pick and shovel, to a point about seven feet below low water, and the bottom being very soft, piles were driven about fourteen feet deep, till they touched good bottom. These piles were driven as closely as possible, and the tops cut off, the dirt cleaned between them for a foot in depth, and three feet of concrete put on top. This concrete was composed of one part of cement, two parts of sand, and four parts of broken stone, thoroughly mixed with water, and put in layers one foot at a time, and well rammed.

All the Monongahela River piers were put in as described, and not a single one even showed the least sign of settlement. Generally the joints of the lower courses open for a while till the crib is thoroughly settled, but in no pier, on any bridge of the P. McK. & Y. R. R., has there even a single joint opened, and there is an immense weight on all the foundations. Take for example this pier No. 5, it has the following weights on the base of the crib:

	Pounds.
Weight from superstructure	440,898
Weight from train	526,400
Weight from masonry	5,668,000
Weight from crib	626,886
Total	7,262,184

or 4,600 pounds per square foot, or 32 pounds per square inch.

The pier is built for a double track bridge, but at present only a single track bridge is built, but with the additional trusses the above pressure will be increased by about 967,298 pounds, or 4 pounds per square inch, making a total of 36 pounds per square inch. I would like to call the attention of the members to Plates 3 and 4, which show the developments of the courses as built, showing the positions of timbers, and the broken stone filling. These plans are not drawn from memory, but are exactly as each stick is laid in the crib. Every crib on the whole road has similar developments, and these are kept in portfolios easily accessible in the main office.

During the summer just passed, the company built a railroad and highway bridge combined, at Broadford, near Connellsville, across the the Youghiogheny River. The water was not over two feet deep in most places, and coffer-dams constructed of cement barrels filled with cobblestones, were built around them, then the material from the river bottom, gravel, etc., shoveled round the outside. This was a capital method. I have heard of empty cement barrels being used in this vicinity for packing behind arches in tunnels, but never for coffer-dams. I used this method for the first time in building the Dawson highway bridge across the Youghiogheny River, at Dawson, Pa., in 1882.

The above three bridges, namely, the Broadford bridge, the Yough bridge, and the Monongahela River bridge were in immediate charge of J. H. Paddock, C. S. Churchill, and H. E. Tripler, and owing to their great care and constant vigilance, there was not an inch variation from the iron work. The Broadford & Yough bridges were not over ¼ of a mile long, but to take the Monongahela River bridge, where Mr. Tripler had

to stake out and locate 150 piers, in a distance of over one mile, and not vary by more than ½ inch is certainly, to say the least, a wonderful performance.

On the discussion of Mr. Brendlinger's paper on the "Foundations of Piers of River Bridges."

Mr. Roberts said: Mr. President, I never heard a more practical paper on local engineering matter than that read by Mr. Brendlinger this evening. There are some points he might have discussed more elaborately, but having so many bridges to deal with, this can be overlooked.

I think our Society should be congratulated on getting so much information about the foundations of bridges built in this locality. I was struck by the fact that in none of the cases mentioned did he have any trouble with quick sands, and I am very glad to know that our rivers are not troubled in that respect.

His experience, beginning at Beaver and terminating at McKeesport, including all the rivers about the city, fixes it pretty plainly that we can build bridges about Pittsburgh without being bothered in this respect, although the foundations are very irregular, as regards depth.

That case at McKeesport of broken stone, or irregular rock foundation, I think is very interesting matter, as is also the point Mr. Brendlinger makes in regard to coffer-dams. I have had a little experience with them, a bitter experience. I have thought also, that dredging and sinking a caisson would be very much better in many cases. I am glad to know there has been so much experience here in favor of that system.

Mr. Ackenheil made a few remarks at the request of Mr. Brendlinger, pointing out the great difficulty in leveling off a pocket crib filled with stone. One of the piers of the Lake Erie Railroad Bridge over the Ohio River, had to be taken down for the reason that a big stone had been left on top of the crib and the caisson with masonry was over six inches out of level when it came to rest on the crib.

This illustrates the experience that cribs with pockets filled with stone will not work to advantage in deep water. That it is better to use solid timber cribs with caissons.

Mr. Brendlinger said: The reason for building pocket cribs is this: It takes very much less timber than solid cribs, and to put in stone in the spaces or pockets, is much cheaper than timber. The timber costs by contract work about $30 per 1,000 feet, board measure: 1,000 feet board measure being equivalent to about three cubic yards of stone, while the stone costs about $3; say $9 against $30, that is, about 1 to 3½.

When you put the solid cribs in yourself, you can get timber delivered anywhere on the Monongahela River or the Youghiogheny River from Connellsville or Pittsburgh, for not over $15 per 1,000 feet. You can put it in place for $5 per 1,000 feet board measure more, about $20, say 1 to 2.

But who is going to spare expense when there is an expensive bridge on top of the crib, that may cost, say half a million dollars, and the idea of running the risk of upsetting the pier or injuring it at all is simply absurd. There is no question that these solid cribs are the cheapest in the long run.

In the cribs on the P. McK. & Y. R. R. we have battered the sides and

not the ends, because what thrust there is, is entirely in the direction of the center line of the bridge.

MR. COLLINGWOOD said: Mr. President, I am a stranger among you here, but as an engineer, I trust I may be permitted to say something on this subject. I came in a little late and did not hear the first part of the paper. The first thing that struck my attention particularly, was the use of rag-bolts. Now, I think that in this generation the rag-bolt is antiquated. Experiments made in the East River Bridge work, show that a straight round bar was altogether better, as such a bolt would often break before it would pull out.

Our practice was to use bolts about $\frac{3}{4}$-inch diameter, boring the holes with a $\frac{1}{16}$-inch drift for the first course, and $\frac{1}{8}$-inch drift for the bottom course, that is, making the holes this much less in diameter. The angles at the ends of the bolts were all slightly rounded with a hammer, so as to prevent any cutting action in driving into the timber. The effect of this is to bend the fibres downward or in the direction driven, and such a bolt will hold more than any rag-bolt you could possibly put in. This is the universal experience of the U. S. Engineers, also. I think there was a paper published some six or eight years ago detailing some experiments in this line, made by government officers.

Another point I would speak on is the pressure on the bottom of these cribs. I think the gentleman has underestimated the safe pressure that can be put on timber foundations. In our work we did not hesitate to use $5\frac{1}{2}$ tons per square foot. His, as near as I can gather, was $2\frac{1}{2}$ tons per square foot. The great thing, is in all cases to insure *uniformity* of pressure. Of course, there are cases where smaller pressures still must be used, but not on hard gravel bottom.

In building the New York approach of the bridge, I made the pressures as near as might be, about 4 tons per square foot, regulated in such way as to make them uniform. That pressure was taken as about the limit, the extreme variation being from about three to five tons.

In sinking the New York* caisson we found that the bottom instead of being level was very uneven owing to projecting points of rock. The variation in level of rock bottom was: from 76 feet to 94 feet below tide. The question was what should be done. The bottom was covered with fine pebbles and occasionally a boulder, and above that was a layer of sand and gravel. At one point under the caisson edge very fine quicksand was found, and at another the edge rested on rock. It was decided to cut the quicksand out for 2 feet depth and fill in with concrete, and to cut the rock out to the same depth below the shoe and fill in with loose sand, then to go ahead and fill the caisson.

That was done and so far as we can tell, the settlement has been absolutely uniform from the time that the masonry reached high water until the total height of masonry was complete. The settlement was almost exactly $1\frac{1}{4}$ inches at every point, showing that if there had been any inequality at all in settlement, it was before we reached high water. The two anchorages rest entirely on sand. Care was taken in these and the ap-

*In sinking the Brooklyn caisson, it was finally stopped on a layer of sand over the northeast corner, while most of the remainder was on an indurated blue clay.

proaches to remove all clay or other compressible material. The New York anchorage settled about an inch more at rear than front, owing to greater pressure per square foot. Since the cables have been built, thus throwing the center of pressure farther forward, this difference has lessened somewhat.

If the pressure is uniform and the bottom of uniform character, you will have no trouble. If the pressure is not uniform and you have a good bottom it is a simple matter to so arrange the foundations as to get a uniform pressure, although it will require some work; but when you get cases where the bottom is of variable bearing power it is a very different matter.

The paper I think is a very valuable one, and I am glad to have had an opportunity to hear it.

Mr. Brendlinger said: I have not used rag-bolts for the last eight years. I found out that rag-bolts tear the timber all to pieces. I have used the bolts described by Mr. Collingwood about eighteen inches long and one inch in diameter. In Montreal there were some docks torn down, and it was found that the round bolts were so hard to get out that the timbers had to be torn apart, whereas the rag-bolts came out very easily. The same thing was experienced in Baltimore docks and for that reason, and from my own experience, I abandoned the use of rag-bolts.

As regards the pressure in those piers not being very great on the foundations. I did not aim to get a very high pressure. I tried to get it as low as possible. I tried to get stability and was very glad to reduce the pressure to $2\frac{1}{2}$ tons.

Mr. Collingwood: There is danger in these rivers of building foundations above low water. The foundations put in under the piers of the Sixth street bridge in 1824 were at least six or eight inches above the surface of the water two weeks ago. I refer to the piers under the old bridge. Those under the present bridge reach about 10 feet below low water, as I have been lately informed by Colonel Roebling.

Mr. Dempster: The foundation for the Monongahela bridge lately built, will compare favorably as to stability, with any of those mentioned by Mr. Brendlinger. Two of the new piers were very near the old ones, especially that of the pier nearest the northerly shore. And much more difficulty was experienced in getting it in place, than could have been where the engineer had full command of the adjoining river bottom. The foundation of the pier spoken of was excavated to a depth of about nine feet below that of the old, and just alongside of it, entirely exposing it to view. The old pier had been constructed on a foundation of four layers of timber, (hemlock I think), laid longitudinally with the pier, with none laid transversely, and had maintained their position for a period of about 65 years. From that as a positive fact and reliable data, not so much is required to ensure stability as might be supposed, but the chief engineer, Mr. Davis, who is here to-night, acting on a safer basis, designed new masonry on more substantial plans, and had the work constructed under his supervision with much more care, and from a comparison of the character of the work of the old and the new, if the old stood for 65 years stable and unmoved, the new will endure for unmentionable time.

Charles Davis said: I do not think I can say anything of special interest to the society. Mr. Brendlinger's paper covers a wide scope. He

has had considerable experience in this line and his work shows good judgment. Mr. Brendlinger should have said something more about the foundations of the Point bridge. One foundation crib had some 14 feet in depth of timber in it. Considering the size, 34 × 92 × 14 feet, it was a pretty big mass to handle.

It was built on the water near the site and floated to place. The platform contained a sufficient number of cells filled with stone to act as ballast in sinking it. It was decked with plank and sided with heavy studding and plank to a height of about 5 feet, and the whole of the sheeting carefully caulked, and made water tight to form a caisson to allow of the lower courses of masonry being laid without the interference of water. After, however, it was made ready and the pit dredged, a flood came, and only with the most strenuous exertions was it saved from being swept away —its great draft and broad exposed face, gave the current a strong hold on it. The pit was filled with gravel and stone by this flood, and the work of dredging had to be commenced again. This made considerable delay. When everything was ready the second time, the platform was anchored over the site and settled to place, and with the result of its being over 1½ feet out of level. Weighting was tried. Dressed stone was closely piled on one part of platform to as great a height as was consistent with safety, and allowed to remain for some time, this expedient diminished somewhat the difference of level. The lower courses of masonry plainly show the irregularity in level. The platform was resting on a number of points with cavities intervening, but owing to the rigidity and homogenity of this volume of timber, a uniform bearing surface for the masonry was obtained in spite of the irregularities of the bed. The piers were built hollow to reduce the weight to the lowest limit. The two piers contain 8,500 yards of masonry.

The north anchorage platform according to estimate record agrees precisely with low water, instead of two feet below it as has been stated. The anchorage platform is some 50 × 80 by some 4 feet deep. Market street gauge was not referred to in contract, or used as datum. Actual low water was taken.

Solid timber platforms when placed deep enough in the gravel or sand to be beyond the influence of scour, make an excellent foundation, always insuring uniformity of settlement, a great desideratum when great weights of masonry are to be sustained, and have an additional advantage of being more economical, generally speaking, than concrete, crib, or pile foundations.

The Smithfield street bridge specifications called for solid timber platforms and required the top level of them to be one foot below low water as shown by the zero of Market street gauge, or in other words one foot below the zero of this scale. Two of these piers stand on deep solid platforms, and the third on account of the interference of the old suspension bridge pier, (this standing in part on the site of the new pier foundation), piling and concrete were resorted to. The contractors, Mr. Alexander Dempster and Mr. Thomas S. Biglow, met with unusual obstacles in the prosecution of the work on account of the interference of old piers, the loose character of material encountered, and the fluctuating stages of water.

The foundations of two of the piers came close to the old ones, and as these had very shallow foundations, they were necessarily, to some extent undermined by the excavations. The contractors, however, succeeded in getting deep, broad, and permanent foundations, and built the masonry of these three main piers to the height of some ten feet above datum, before the ownership of the bridge changed hands. One foundation had ten feet in depth of timber in it, another eight feet. The third was excavated to depth fifteen feet, then piled, and the pit partially filled with stone, and the whole covered. There was more difficulty connected with these foundations than on the Point bridge, or probably any other bridge on the two rivers.

The experience gained at the Point Bridge was, however, of some service here. Solid timber foundations for our rivers in this section, at least, are the best, except in special cases, considering the facility with which they can be built under ordinary circumstances, and their comparative small cost. The item of the saving in time is a very important one.

In the Cincinnati suspension bridge, solid timber platforms were used for the foundations, these platforms were carried up to plane of low water, and masonry started at that level*. The results obtained there with solid timber are very satisfactory.

Mr. Roberts said: I would like to cite an instance illustrating the benefit of timber foundations. I had occasion to put a pier in the Conochagague Creek, near Chambersburg, in the Cumberland valley. The region is of limestone formation. The rock crops out over the surface at frequent intervals, but it may some time occur that excavations of 20 feet or more depth may be made in close proximity to ledges through soft material.

In this particular case, after excavating inside the coffer-dam, only 3 feet below the surface of low water, we struck solid rock at each end of the pit. But there was an intervening space of about 10 feet between the rocks and just about the middle of the pier, which indicated a quick-sand for at least 18 feet depth. I did not test it below that depth. I concluded that it would be safe to bridge this gap with three courses of timber, and did so. The pier built over it was about 30 feet high, and has been standing now three years with, so far as I know, no signs of settlement. Strong timbers below water surface, laid in courses, in different directions, is in my estimation a safer plan to distribute weight than to make the sole reliance a bed of concrete. In the instance I have cited here concrete alone could not have been applied.

*See John A. Roebling's Report on Cincinnati Bridge, submitted April 1, 1867.

ABOUT SOME OF THE PROPERTIES OF STEEL.

By Alfred E. Hunt, Pittsburgh, Pa.

A paper read before the Engineers' Society of Western Pennsylvania, December 18, 1883.

It is the object of this paper to discuss some of the properties of steel, and to give the writer's opinion as to the desirable combination of these properties in steel for some specific purposes.

According to the latest definitions, unofficially accepted by the various engineering societies, and adopted in the United States tariff laws, steel includes a material as soft and pliable as any wrought iron, or as hard as flint; a material of an elasticity of the famous Damascus blades, or as brittle as glass; a material with 25 per cent more ductility than the best iron, or one of two hundred thousand pounds tensile strength to the square inch, and almost no perceptible elongation; a material that will weld nearly as well as the best wrought iron, or a steel differing from chilled cast iron, only in its malleability; a material varying from almost nothing upwards in any of all these properties.

It is this last property alone of *malleability* which, although it may vary, must still be clearly manifest, which limits, and with the short word *cast* before it (describing the method of producing the metal) defines among the iron containing metals, that much used and also abused material, *steel*, according to the definitions of our savans.

It may be mentioned here that a definition that in concisely describing steel shall include every possible variety and exclude everything else, has been found to have been a troublesome undertaking, and that among the aforesaid savans two of our members have taken very prominent parts, and have aided a great deal in shaping these definitions for both engineering societies and the Government officials. I refer to our ex-president, Mr. Wm. Metcalf, and to the late Mr. James Park, Jr.

Although these properties of malleability, brittleness, elasticity, tenacity, ductility, pliability and hardness, are all modifications of the molecular force, called *cohesive attraction*, they are nearly opposite properties, and may be crudely divided, as those going with hardness and those with softness and pliability.

In good steel these properties are in well defined relations and proportions to one nother, increasing in the amount of one property and bringing a corresponding decrease in the opposite quality, as, for instance, hard steel of over two per cent carbon is very difficulty malleable, and soft low steel is as easily malleable as the softest wrought iron, but has almost lost its power of being hardened, and can be whittled with a stout knife almost as easily as lead.

The chemical composition has much to do with the preponderance of either set of these qualities; in fact, good steel for any specific purpose is made of a certain chemical composition within quite close limits, which limits are generally narrowed accordingly as the physical requirements are more numerous and difficult, but not necessarily because a steel has the required formula of chemical composition is the metal satisfactory. It may not have been properly made, it may not have been properly heated, or worked into the required shape, in short, it may have fully as many physical causes of failure as it has chemical.

A malleable metal is one that can be reduced in section by hammering or rolling. The malleability of steel depends largely on its chemical composition; the less foreign ingredients, except in the case of manganese, the more malleable. Steel of less than 0.06 per cent carbon becomes so oxidized generally as to have its malleability seriously diminished, but can be forged into bars and shapes with over three per cent carbon. Manganese, up to one and a-half per cent in steel, made by the open-hearth and Bessemer processes, increases its malleability. Venadium, titanium, chromium, aluminum and wolfrum have each been alloyed with steel in varying amounts up to ¾ of one per cent, to obtain a hardening effect combined with toughness, and without much injuring the malleability of the metal.

The changing of the relative situation of the atoms of a steel ingot by reducing it under a hammer or between rolls while the metal is above a red heat, gives it a much stronger cohesive attraction.

The denser and more compact the steel is formed in an ingot, the less of this reduction, technically called "work," will be required. Ordinarily, the lower the carbon the more oc-

cluded gases are left, as blow-holes in the ingots; high crucible steel, however, has a peculiar structure at the top of the ingot, called "piping." All these open structures can be almost entirely eradicated by placing a heavy pressure upon the molten metal immediately after casting, and many devices have been tried to secure this end. In some recent work by Mr. G. W. Billings, of Cleveland, O., he has placed a pressure of 1400 pounds per square inch upon the molten metal, and has succeeded in producing perfectly sound ingots of crucible steel from top to bottom, where the other ingots of the same grade of material melted at the same time show the characteristic piping. This seems to the writer to be a step in the right direction, and points to a way of avoiding the open structure of steel ingots, which is one of the greatest sources of weakness and which necessitates a large amount of work to be overcome. Ferro-silicon, an alloy of iron, manganese, carbon and silicon, is added as a physic to steel to produce sound ingots. This method leaves about one-quarter of one per cent of silicon in the metal, which is rather a source of weakness, but is well adapted for the purpose for which it is coming into general use—the producing of solid steel castings, especially by the open-hearth and Bessemer processes. Ordinary Bessemer and open-hearth steel ingots need to be reduced to at least one-tenth their sectional area before showing their maximum cohesive attraction. Thus a steel forging of 18 inches diameter needs to be reduced from an ingot of at least 57 inches diameter in order to have the steel get its proper work, but here comes other difficulties of uniformly heating such a large ingot so that the center will have as much heat as the outside, and of obtaining a heavy enough hammer to penetrate and work the interior of the ingot. To properly forge an ingot of fifty seven inches diameter a fifty-ton hammer is required, and the fitting up of a fifty-ton hammer plant is, to say the least, a ponderous task. The writer believes that the best way to make large steel forgings is to make them of about 0.20 per cent carbon and 1.00 per cent manganese steel, which has been cast into fourteen inch ingots rolled to muck bar of six by one inch and piled and welded as in iron forgings. This steel will stand the heat and can be welded about as well as iron, and though having the same objection of welds as in iron forgings will have the superior tenacity and ductility of steel over iron. Ordinary crucible steel requires to be reduced to about one-quarter the original area of the ingot.

An elastic metal is one that will resume its original form or shape after having been extended, bent or compressed by some external force. Different steels possess this property of elasticity in very different degrees. The limit of elasticity is generally spoken of as the number of pounds of strain per square inch that a sample will stand and again resume its original shape after the strain has been removed. The softer and lower the tensile strength of a steel the lower the limit of elasticity; in the softest steel the tensile strength is about fifty thousand pounds per square inch, the contraction of area about seventy per cent and the limit of elasticity about twenty-five thousand pounds per square inch; in the highest tempered steel the tenacity runs up to two hundred thousand pounds per square inch, the contraction of area reduced to almost nothing and the elastic limit raised to very near the ultimate strength, say one hundred and ninety-five thousand pounds per square inch.

A hard steel is one in which the particles resist impression, separation or the action of any force which tends to change their form or arrangement. All these modifications of cohesive attraction which give rise to the properties of malleability, ductility, tenacity, hardness, etc., are dependent upon the particular form of the atoms of the material and the particular manner in which they are arranged. The arrangement and form of the composing atoms of steel can be changed by heating it red hot and then cooling it either slowly and allowing the crystals to arrange themselves naturally, as it were, or very quickly allowing but little time for this molecular arrangement; in the one case we have an annealed material with superior properties of the soft, pliable and ductile division to the original material; in the other we have a hardened steel of superior tenacity and properties of the hard order, but with corresponding sacrifice of the soft qualities of ductility, etc. That there is an entire difference in the arrangement of the composing atoms between ordinary steel and the same material hardened can be readily perceived by examining the fractures of the two specimens, also by noting the increased size of the hardened steel. There is, too, a chemical change in the condition of the carbon in hardened steel; this fact is not generally understood. It has a practical bearing in the estimation of carbon by the color method, for the condition of the carbon is so changed that it does not yield to the same extent the characteristic brown color to a nitric acid solution of the steel upon which the color carbon method is based. Hardened steel always yields a lighter color, and gives varying results from five to thirty per cent less of carbon by the color method than actually exists in the steel as obtained by gravimetric methods of analysis or as obtained by the color method from the same material in its natural condition.

Open-hearth and Bessemer steel begins to show material hardening in the fracture at about 0.30 per cent carbon, and in one-half inch square bars will fly with a sharp blow of the hammer. In open hearth steel there

is a considerable variance in hardness beyond that given by its chemical composition by the character of the stock from which the metal is made. The best results for soft boiler plate and other steels where softness is one of the leading requirements cannot be had where a great deal of high steel scrap is one of the components of the metal. Also the different character of the flame used in the open-hearth furnace alters the hardness. Steel made by a smoky reducing flame is generally harder and at the same time has, proportional to the increased tenacity, more ductility than steel made of exactly the same chemical composition made with a cutting oxidizing flame. For these reasons two heats of spring steel with, say 0.75 per cent carbon each, and of exactly the same chemical composition, may appear like steel with fully 0.10 per cent carbon difference, it being due in the one case to the use of steel scrap high in carbon and manganese in its composition and in the other to low material, and in the one case the reducing flame being used in the melting furnace, and in the other a sharp oxidizing flame.

Manganese is a hardening element in steels. This is especially noticeable in the open-hearth and Bessemer metal, where it often exists in amounts of one per cent and over. Over three-quarters of one per cent of manganese in spring steel will make it brittle and liable to break after hardening.

Steel, of no matter how low carbon, will harden somewhat, as shown by increased tenacity and decreased ductility in the suddenly cooled steel. On this account, and because the higher steel becomes more rapidly crystalized and brittle under repeated shocks and vibrations, and also because, the writer thinks, in all the soft structural steels, it is more the increased ductility rather than the increased tenacity over iron that gives soft steel its superiority, he believes that for boiler plate, steel chain, ship plate, and the like material it is far better to use a steel of not over sixty-five thousand pounds ultimate strength, with a contraction of area of from fifty-five to seventy per cent, and with an elongation of from twenty-five to thirty per cent in eight inches, than if made of material of over sixty-five thousand pounds per square inch tensile strength, and with a decreased ductility, although this ductility may be in good proportion to the increased tenacity.

The writer believes the United States Government laws allowing marine boilers to be made of less thickness, or to carry a higher steam pressure, accordingly as the tenacity is higher, and with a correspondingly sacrificed ductility, to be wrong, and that the thinner shell and higher steam pressure should be allowed accordingly as the ductility is greater with a tenacity of not less than 55,000 pounds nor more than 65,000 pounds per square inch.

A flexible metal is one that will allow considerable motion of the particles on each other without breaking. The flexibility of steel is usually tested according to the English Admiralty test by bending it around a bar of diameter equal to about twice the thickness of the rod or plate to be bent, and noting the angle through which the specimen is bent before rupture, or in cases of very soft steel, by bending it backward and forward through an angle of ninety degrees, alternately each side of the perpendicular. Soft steel of 0.10 per cent carbon rolled into thin plates and horseshoe nails stamped therefrom will, after annealing, bend thus from six to eight times before rupture. The best quality of Bessemer and open-hearth steel forged into bars of two by five-eighth inches and annealed will bend over double upon itself without breaking or shearing up to about 0.55 per cent carbon, and is the test of the 0.55 per cent carbon material from which locomotive tires are made.

A tenacious material is one that resists separation in the direction of its length. The tenacity of steel is generally spoken of as the number of pounds pull it takes to the square inch of its section to rupture it.

The ductility of steel is usually spoken of either in the terms of its percentage of elongation or the percentage of contraction of area of the section of the ruptured specimen. As the percentage of elongation in the same material varies inversely to the length of the specimen pulled apart, the percentage of reduction of area is a more uniform quantity, and is coming more into use for measuring and comparing the ductility of different samples of steel.

Unless directly specified as being computed upon the contracted area, the ultimate strength and elastic limit is understood to be calculated upon the original area of the section.

Some requirements demand that the ductility shall be measured by the percentage of elongation or contraction of area at the "*moment of failure*," or the instant that an increase of pressure ceases to be recorded by the testing machine, as usually a very considerable flow of the metal and extension takes place after this "*moment of failure.*" This method of obtaining the measurements gives results about 25 per cent less than those obtained in the usual way, and does not give correctly the true ductility of the metal, as the elongation and contraction will be less, accordingly, as the limit of elasticity is nearer the ultimate strength.

Many orders for steel are placed with requirements for a given tensile strength per square inch, and perhaps with a given elastic limit, but with no requirements for a corresponding ductility. Ordinarily, both the tensile strength and the elastic limit are higher

in a poor steel made from an inferior grade of stock, and containing more impurities, as notably phosphorus, than in good steel of the same percentage of carbon, so that the fulfillment of a requirement for a given tenacity and elastic limit alone is not a criterion of the quality of the steel.

DISCUSSION

OF MR. HUNT'S PAPER "ABOUT SOME OF THE PROPERTIES OF STEEL."

Proceedings of Engineers' Society of Western Pennsylvania.

MR. METCALF said :—I do not know that I have much to say, Mr. President. I think most of the positions Mr. Hunt has taken are very good. The paper is so much a text that it is almost impossible to discuss it in detail. One or two points spoken of struck me particularly: one in regard to the piping of the ingots. In making the finer steels piping can be obviated to some extent by pressure, but I think if any gentleman will try it carefully in the small ingots that are usually required for tool steel, he will find that the expense of reducing the pipe by mechanical means is greater than by knocking the end off with a sledge. It seems like a very serious loss, and so it would be if all the work of finishing had been done on it. But the pipe should be knocked off before the price of skilled labor is put on it; you have then only to bear the expense of remelting the pipe part, but you cannot avoid the necessary expense of applying mechanical pressure of any kind when it is spread over several hundred small ingots in a day. The great difficulty is in keeping the ingot molds so that they will resist such pressure. You will find, after all, the pipe is the cheapest, if the steel is well melted.

In regard to large ingots, a little care in teeming them will prevent any excess of piping.

One other matter Mr. Hunt spoke of, in reference to that large ingot. I have no doubt that the calculations are entirely correct. To make an 18 in. shaft, with the work proportioned to what we ordinarily do in making small bars, would require a 57 in. ingot, but I doubt very much whether in making a large structural mass like that, it is necessary to reduce the whole grain of the mass to the fine condition necessary for fine tool steel. Tool steel, of course, is necessarily fine-grained. A good piece of steel, when it is tempered for that purpose, should be so fine that the grain is imperceptible to the naked eye. But that condition, I cannot conceive, is necessary in large structural steel.

Now, if we take into consideration the experiments of Tchernoff, the Russian engineer —he went into the matter very thoroughly in the gun foundry in Russia—he made some very beautiful experiments several years ago, which were beautifully illustrated in *Engineering*, and his work is a classic on steel. It is well worth anybody's time and trouble to hunt it up and study it. He made this beautiful illustration of the fact of the crystallization of steel in cooling. He said that taking a solution of some salt of alum, I think it was, at any rate one of the crystal salts, in settling from solutions, the crystallization being allowed to go on quietly, the crystals were extremely large, but if, when the precipitation or crystallization commences in this solution the vessels were vibrated but a little, the crystals were all formed much more uniform in size and much finer and smaller, and so it goes on when a large mass of steel or any molten metal cools. If it cools slowly, as it does in an ingot, and especially in the interior, these large crystals form in the center, which you will always find in any largs mass of iron, whether cast, wrought, or in steel, and the only way to overcome the excessively large crystals, which are elements of weakness in the mass, is to reheat the ingot thoroughly, heat it through and hit it hard. It should be heated to an orange heat, say a lemon color, heat it uniformly through and hammer it very quickly and under a heavy hammer. My impression is that what Mr. Hunt says is all true, yet it must be borne in mind, that for instance, an 18 in. shaft could be obtained from a 24 in. ingot, if handled in that way, viz., well heated, heated soft and then hammered under a ten-ton hammer, especially with rounded dies, swedge dies, and hit quick and hard.

You can get in this way all the results that are necessary in a large structural mass, so that I doubt whether the argument of Mr. Hunt's as to the common practice as applied to tool steel, is necessary at all in the making of large masses for structural purposes.

Those are the only two points that struck me as calling for any criticism. I was much interested in the paper and I expect to enjoy it very much in reading it over.

MR. CURRY said :—I do not know that I have anything to say. I came here to hear the paper read and get instruction from it. Yet with the desire of getting some information on the subject I would like to ask Mr. Hunt in regard to Mr. Billing's experiments. You say that the pressure was 1400 lb. How was that determined?

MR. HUNT :—That was simply estimated.

MR. MILLER said :—Mr. Chairman, I do not pretend to know a great deal about steel, but

I have hammered considerable in the last few years, and I think if Mr. Hunt's theory was to be carried out about those large ingots there would have to be some larger hammers built. I am manufacturing some steel crank pins at the present time, 18 in. in diameter, and I thought if I got ingots 24 in. in diameter that I would give as much hammering as would be necessary, for the simple reason that if I had had larger ones they would have to be heated so much the more, and that I thought would do more harm to the steel than benefit. I thought 24 in. ingots, well heated, to be the correct thing—heat it slowly—and I think we can make very good crank pins with the 24 in. ingots.

I have repeatedly had some large ingots where we were necessitated to draw one part down very small and the other part had to be left large, and I have several times experimented with them to find out whether the small or the large part was the best, and so far as my knowledge goes I cannot see a great deal of difference whether it was hammered so excessively or not.

Mr. Gottlieb said:—With reference to hammering this steel I would like to ask Mr. Hunt upon what rule or from what experience he would support his theory that he would have to take an ingot 57 in. in diameter to make a shaft 18 in. in diameter. I have seen something of that this summer. I had occasion to visit Mr. Krupp's works, at Essen, last August, and saw them forging a steel block 5 ft. by 6 ft. and about 18 ft. long. I saw that piece of steel taken out of the heating furnace, brought under a 50-ton hammer and forged down. I afterwards saw a finished gun, of such size as the block was intended for. It was for the Chinese Government, and that gun was about 48 in. in diameter at the breech end, so it apparently did not take more than 5 ft. to reduce it to 48 in. and probably to 54 in., because the gun I saw finished was turned off, and there must have been something for waste. The hammer was a 50-ton hammer, the blows were very slow and very few on that block. Then they brought it back to the furnace again. The steam hammer was rather a disappointment to me because I had been led to believe the steam hammers were on a similar pattern to those we have here, with steam pressure on top. But it was simply a drop hammer, for when the hammer was lifted up the steam escaped and the hammer drops back by its own weight. It is very slow, and they cannot strike very many blows before the steel gets cold. But I should like to have Mr. Hunt give the information as to forging an 18 in. shaft from a 57 in. ingot.

Mr. Hunt said:—I wish to speak with a great deal of deference to the much more extended experience of the gentleman who have just preceded me. I fully agree with Mr. Metcalf that structural steel ought not to be required to have the same fine grain as crucible or tool steel. The point in the paper was that open-hearth and Bessemer structural steel required to be worked down to about one-tenth the area of the cross section of the original ingot in order to have the material show its best results, and that one of the most common sources of failure in large steel masses was that it had not received a sufficient amount of work.

It is a fact that some of the engineering societies of the continent have recently condemned steel for large shafting, and steel is getting into bad repute for such purposes in many places in this country on account of its failing—breaking short off while in service. I have examined some of this shafting of from twelve to fifteen inches in diameter broken thus short off like a pipe stem and have found that the fractures showed no seams or flaws, the material being sound throughout; have analyzed the steel at several places in the fractures and have found it to be homogeneous and all right, the fracture however showing large unworked crystals in the center which grew finer towards the surface.

In the testing machine steel shows this lack of work very markedly. For an illustration I will cite the results upon some steel ingots of carbon 0.28 per cent and manganese 0.60 per cent. An ingot of 24 inches diameter was forged to 16 inches diameter, and test pieces taken from disks cut from the forging. A part of the forging was reduced down to eight inches square and test pieces taken in like manner from it; another portion of the same material was rolled into a plate and test pieces taken from it as well. These test pieces were broken in the testing machine, the test pieces slotted out of the forging at 16 inches diameter broke off short at 60,000 pounds tensile strength, and with less than five per cent in reduction of area and with a granular brittle fracture. The test pieces taken from the eight inch square block of the same steel ran up to about 75,000 pounds tensile strength per square inch, the contraction of area increased to about 14 per cent and the fracture very much less granular and brittle in appearance. The test pieces taken from the plate of the same material rolled down to about $\frac{3}{8}$-inch went up to 85,000 pounds per square inch tensile strength and the contraction of area increased to 18 per cent, and the fractures showed fibrous and tough.

These tests are only typical illustrations. I have made many such with the same results and I think it is a universal experience to find steel to give better results as it is properly worked down, and until ordinary steel has had considerable work, much more than the preceding gentlemen have mentioned, that it breaks off under strain in the testing machine short and brittle and the fractures have just the appearance that the fractures of the

large steel shafting material has which has failed in our river boats and the like. I agree with the gentlemen in the difficulties of heating and forging large steel ingots, and have in fact mentioned them in the paper.

It was in this connection that, I believe, Mr. Billing's compression process to be a step in the right direction. I think that Mr. Metcalf will find upon investigation that the expense, after the first cost of the apparatus, of compressing several hundred small ingots per day will not be so expensive as he apprehends, but it is not in the saving of the pipe, however much of an advantage that may be, that I think the chief use of the compression process will be, but it is in so pressing the molten metal as to drive out the occluded gasses, reduce the size and number of the blow holes, and so solidify the ingot as to do away with this necessity for so much work.

Mr. Metcalf said:—It seems to me in the first place it is impossible in any large forging of that kind to bring the strain on the whole mass to such a degree as to require the same strength in the center to resist the strain that you do on the outside. Take the case of a crank pin or any piece of metal like that. If you have the outside for a small distance in of the tenacity of 75,000 or 80,000 lbs. you have a very safe shaft. Then if the shaft in the interior is not sound by reason of bad working or careless heating, if it is subjected to any strain it will break, and it will start to break on the outside, the limit of the strain of any piece being of course the strength of the weakest part. The strain, I take it, in any large mass of that kind is brought on a very small part of the mass first, and if there is sufficient material back of it, or stiffness to do the work required you have all that man can get.

I have a case in mind that occurred in our works that is perhaps illustrative of this whole question. There is there a 9 in. shaft, 8 in. journal, 16 in. long. It carries an 8 ft. pulley, or did at the time I am speaking of, of 2 feet face, and it was run by a six-ply gum belt, 2 feet wide. This drove a 9 in. mill at one end with a speed of 225 to 250 revolutions. At the other end it drove a 14 in. mill, which frequently pulled out strands 5 or 6 in. wide, down as thin as 18 gauge and 150 feet long. You will see what a strain there was on the shaft. I want to tell where the shaft came from, because I believe it was sent us as a joke by our friend Jones. We sent for a large ingot to make that shaft. After that shaft was forged and finished we found a pipe right through it from one end to the other big enough to drive your fist through. Mr. Parkin called my attention to it and we looked at it, and as we had some experience in pipes and much sympathy for them we concluded to put it in. After it had been run for some time, the engine ran away. The engineer ran out to shut the steam off, and while he was doing so one of the boys thought he would help matters by making a pass through the rolls. The result was there was not a piece of those fly wheels left more than 3 or 4 feet long. A large hole was cut through the iron roof. After the dust had cleared away we made an examination. We examined this shaft and found that neither it nor the shaft of the engine were sprung. We examined the counter shaft and it was not sprung in the slightest degree. We put on an 8 feet pulley with 3 feet face with a 3 feet gum belt, and we are driving the same mills with it today, and that shaft will drive anything you can get into the rolls.

That illustrates my point, that I do not believe in the necessity, except in a gun for instance, that the material need be fine-grained. You must simply regard the initial strains that act upon the surface of it, provided the whole mass is free from interior flaws, due to uneven or irregular heating, or work of that kind. Therefore, I can not believe there is any necessity for the excessive amount of forging that Mr. Hunt speaks of, although I do not question his figures at all. The results as he gives them are such as I would expect to find, yet where the tenacity is 75,000 or 80,000 lbs. at the outside, that would be the useful strength of that forging if the whole mass were put into use without any disastrous strain.

Mr. Gottlieb:—Those tests Mr. Hunt has stated, made from different diameter of shaft, may be correct, but I share the opinion of Mr. Miller and Mr. Metcalf, that excessive heating and forging may produce more harm than good, by just creating such unequal strains as Mr. Metcalf refers to. Now, it is a question whether or not it is necessary to reduce to such an extent as Mr. Hunt states, in order to get the best results.

Mr. Roberts:—I would like to ask our Honorable President in regard to the hammer he saw in Germany. I understood that the 50 tons were lifted and dropped, something like a pile driver?

Mr. Gottlieb:—It had a drop of about 12 feet.

Mr. Roberts:—I have seen in the newspapers we have the heaviest steam hammer for forging used in the United States, constructed chiefly for the purpose of making steamboat shafts, and I would like to know how that weight of hammer compares with what we have in Pittsburgh. Can any of the members tell how our heaviest hammers here compare with the 50-ton hammer in Essen?

Mr. Gottlieb:—Our heaviest hammer here is at Park's, 17 tons.

Mr. Hunt:—The usual pressure is about 100 lbs., but it is an initial pressure. Diameter of cylinder is 42 in. with 9 ft. stroke.

ANNUAL REPORTS OF OFFICERS.

ADDRESS OF THE PRESIDENT.

To the Engineer's Society of Western Pennsylvania:

At the expiration of the second term of office during which I had the honor to preside over your meetings, I avail myself of the time-honored privilege of addressing you to review briefly the noteworthy incidents of the past year, and submit to you a report of the present condition of our society.

Flourishing and prosperous as the society was, when I was the first time honored with the office I am to vacate to-day, I am much gratified to state that it has grown and developed, both in numbers of membership as well as in efficiency, during the past two years, mainly through the zeal and efforts of its own members, who manifested an unprecedented interest in the transactions of our meetings and have placed our society into the enviable position to have to-day the best frequented meetings of any similar society of this country. This is the best proof of the usefulness of our society, and the best guarantee for its future existence and prosperity.

The nine regular monthly meetings were frequented, on the average, by 53 members, which is 20 per cent. of the membership; the total number at present being 255, a net gain of 16 over the past year. The society mourns the loss of 2 worthy members by death, 18 have resigned, and 13 have been dropped from the list. Engineers' societies, unlike other professional bodies of similar purpose, are composed of members whose field of operations is the whole world: their avocation compels them to leave one place and settle at another, wherever public improvements or constructions require their service. This will explain the number of names annually dropped from our list—the very nature of the engineer's life and pursuits is the cause of it.

Nevertheless the new acquisitions overbalance the losses, which is very encouraging. Considering further the quality of the newly-admitted members, not a few residing in distant states, we cannot but consider this fact as an endorsement and appreciation of our labors and the results achieved. Located, as we are, in the center of large and varied industries, our members most actively engaged or connected with the same; some of them identified as the chief promoters of the vast progress made during the last twenty or thirty years, and who does not know and admire the progress in that period, the character of the papers read in our meetings and the discussions following, were mostly of a practical nature, imparting generously and unselfishly experiences gained through years of intelligent observations and comparisons, freely placed at the disposal of the profession in general, and our members in particular.

This I think has been, and ought to be in the future, our main object and strength. Such information cannot be obtained from books, at least not from any one book, and no place is better adapted for and better

supplied with scientific and practical minds to collect interesting and important information than ours, and hence we may reasonably expect to be always able to contribute a welcome share of knowledge to the profession at large, and that the fruit of our labors will be more and more appreciated. This leads me to hope that a much-cherished wish on our part, to which I have given expression in my last annual address, but the realization of which to see was not destined to my term of office, may ultimately, under more favorable conditions and at more prosperous times, yet be fulfilled at a near future. I mean, of course, the acquisition of permanent quarters for our society. I warmly recommend to my successor in office not to lose sight of this important object, and hope that he and the incoming board will be more favored by circumstances than I was.

The room at our disposal heretofore for our meetings was uninviting and rather an obstacle to the advancement of our society than otherwise. The present locality is a decided improvement, yet not what it might be; still it is the best the board of directors could furnish at the present time, and I fail to give expression to the sentiments of the members by rendering the thanks of our society to the members of the Western Iron Association for the kind and noble spirit with which they placed their assembly room at our disposal.

To the board of directors, officers, and members of the society, I beg to express my sincere thanks for their kind support and for the uniform courtesy I have been treated with, and asking for your pardon for the shortcomings on my part, I hope to be excused on the plea that I have tried to do my best, as best I knew, and that willfully I have not neglected any of the duties of the honored trust you have placed in my hands.

Wishing the best success and prosperity to the society for the future, I am,

Your retiring president,

A. GOTTLIEB.

REPORT OF THE TREASURER.

For the year ending January 15, 1884.

1883.	RECEIPTS.		
Jan. 16.	Balance	$ 518.86	
	Dues from 2 member to January 17, 1882, at $5.00	10.00	
	Dues from 36 members to January 16, 1882, at $5.00	180.00	
	Dues from 2 members to January 16, 1883, at $2.50	5.00	
	Dues from 228 members to January 15, 1883, at $5.00	1,140.00	
	Dues from 17 members to January 15, 1884, at $2.50	42.50	
	Advance payments	10.00	
	Publications	19.50	
	Total		$1,925.86
	EXPENDITURES.		
	Mercantile Library Association to December 15, 1883	400.00	
	Salary of secretary, one year	200.00	
	Re-printing transactions	633.25	
	Preparing Library Catalogue	100.00	
	Printing " "	147.25	
	" Transactions	82.50	
	" Postals, etc	62.50	
	Postage and stationery	74.30	
	Insurance on Library	30.00	
	Telegrams on excursion	6.20	
	Stenographic reports	5.00	
	Total		$1,741.00
	Balance in hands of treasurer		184.86
			$1,925.86

Respectfully submitted,

ALBERT E. FROST, Treasurer.

Library Fund.

1883.	RECEIPTS.		
Jan. 18.	Balance	$ 40.28	
			$40.28
	EXPENDITURES.		
	Books, as per vouchers on file	37.30	
	Binding	1.20	
	Binding	1.00	
	Total		$39.50
	Balance in hands of treasurer		1.78
			$40.28

Respectfully submitted,

ALBERT E. FROST, Treasurer.

REPORT OF SECRETARY.

PITTSBURGH, Jan. 15, 1884.

To the Engineers Society of Western Pennsyleania:

GENTLEMEN:—During the year that has passed 54 new members have been received into the society, on payment of dues, making, with the 239 on the books at the last annual report, a total of 293.

The society has lost 2 members by death, 18 have resigned, and 13 have been dropped for non-payment of dues for two years, and all trace of 4 has been lost, leaving now on the list 255, a net gain of 16.

Nine regular meetings were held as follows:

	MEMBERS.	ATTENDANCE.
Jan. 16	239	42
Feb. 20	255	43
March 20	262	60
April 17	269	46
May 15	270	69
Sept. 18	277	44
Oct. 16	285	58
Nov. 20	295	65
Dec. 18	295	48
Total		475

Attendance 20 per cent.; average 53.

The average attendance has been the best of any year except the first. The average attendance in 1880 was 32 per cent. of the membership; in 1881, 18 per cent.; in 1882, 17 per cent., and in 1883, 20 per cent.

The following papers have been read:

Feb. 20.—"The Gravity Inclined Plane," by S. B. FISHER.

March 20.—"Some Causes of Cold Shortness and Red Shortness in Iron," by WM. METCALF.

April 17.—"Evaporative Tests of Steam Boilers," by WM. KENT.

May 15.—"The Monongahela Suspension Bridge," by S. M. WICKERSHAM.

Sept. 18.—"The Highways of the People," by S. B. FISHER.

Oct. 16.—"Foundations for River Bridge Peirs," by P. F. BRENDLINGER.

Nov. 20.—"Boiler Explosions," by JOS. L. LOWERY.

Dec. 18.—"Some Properties of Steel," by A. E. HUNT.

These have been printed and distributed except those of Oct. 16 and Nov. 20. The former is in the printer's hands, and the discussion of the latter is in the hands of one of the members for correction. We have had a stenographer at the last three meetings, who has taken full notes of the discussion of the papers, which he has written out and promptly sent to the secretary.

The finances of the society are in a favorable condition, of which the report has been left to the treasurer.

More dues have been collected this year than ever before.

The board of direction has held nine meetings during the year.

The committee on rooms has made arrangements to meet in the rooms of the Iron Association, 77 Fourth Avenue, hereafter, and the library of the society will be moved to room No. 7 in the same building as soon as suitable cases are made.

The *American Manufacturer* has published as heretofore the papers of the society, except two which were printed by *The American Engineer.*

The society in exchange for its publications has received the following papers:

1. "Journal of Society of Arts."
2. "The American Engineer."
3. "Journal of Associating Engineering Societies."
4. "Norsk Tecknish Tidskrift."
5. "Annals de la Sociedad Clintifica," Buenos Ayres.
6. "Ingenious Foreningens, For Handlingen," Stockholm.
7. Revista de Abras E Minas," Lisbon, Portugal.
8. Die Anlarge Betrieb der Eisenhullen," Liepsig.
9. "Transactions of the American Society Civil Engineers."
10. "Proceedings of Engineers' Club of Philadelphia."
11. "Report of the Institution of Civil Engineers," London.
12. "Railway Review."

IN MEMORIAM—ROBERT CRAVATH.

TO THE MEMORY OF ROBERT CRAVATH, CIVIL ENGINEER, THE FOLLOWING LINES ARE DEDICATED.

Your committee appointed at the meeting of the Engineers Society of Western Pennsylvania, December 18, 1883, with the view of drawing up suitable resolutions on the death of our fellow member, considered it well to discard business-like resolutions, and give you instead a narrative, showing in this short biography the life and shifting career of a railroad engineer, the ups and down, the hard work connected with our profession, and the esteem and love an engineer may gain by following his profession in a conscientious way, as did Robert Cravath. The older ones among us, having gone through similar experiences, will find it but a familiar picture; to the younger ones it will show what is before them, and will be a good example for them to imitate.

Robert Cravath was born in Syracuse, N. Y., February 18, 1832. His father was Austin Cravath; his mother's maiden name was Charlotte Hoyt.

Educated at Homer, N. Y., he went to Detroit in 1853 to enter upon his first professional work on the Port Huron & Detroit Railroad. In 1858 he was on the engineer corps of the Illinois Southern Railroad, a line projected from Vincennes, Indiana, to Cairo, Illinois; he was afterwards chief engineer of the company.

While connected with the Illinois Southern Railroad, and residing at Mt. Carmel, Illinois, Mr. Cravath was married, October 23, 1860, to Miss Rebecca Jane Bell, who, with two children, survives. Mrs. Cravath is a daughter of General Hiram Bell, an old and prominent citizen of that place.

In 1863 to 1864 Cravath was engaged with the Ohio & Mississippi Railroad, with headquarters at Cincinnati. In the fall of 1864 he accepted the position of division engineer on the Boston, Hartford & Erie Railroad, removing to Woonsocket, R. I., which position he retained for some time after the suspension of work, leaving it in 1867 to go into business with his father in Minneapolis, Minn., where he remained one year.

In 1869 Cravath accepted the position of chief engineer of the Alabama & Chattanooga Railroad, a line about three hundred miles long, some fifty miles of which were finished. This road included the Wills Valley and the Northeast and Southwest Alabama Roads, and was completed by Mr. Cravath, who, under the orders of the board of directors, had full charge of the work. By his energetic and able management, combined with thorough engineering ability, he was enabled to finish the work in a very short time, despite many obstacles and difficulties both in engineering work and financial arrangements. While building this road Cravath had to act for a great part of the time as paymaster, carrying along the line large sums of money; then as purchasing agent, buying

all track materials and the equipment of the road. This, coupled with securing the rights of way, laying out the line, designing and supervising important bridges, buildings, and the general construction of the road, with but little trained assistance or advice, put Cravath on his mettle, showing him to be a good manager and able engineer. Before the completion of the line he was made general superintendent, which position he occupied for two years, until the culmination of the financial difficulties, which had surrounded the enterprise from the start, brought the road to a stand-still in 1871.

When this occurred Mr. Cravath came north and took charge of surveys for a new line in Ohio, under the auspices of the Pennsylvania Company. Early in 1873 he came to Pittsburgh and remained with the Pennsylvania Company until 1880.

During his service with the Pennsylvania Company a large number of improvements were made on their lines, and new projects frequently made it necessrry to investigate engineering questions involved.

Under the directions of the chief engineer of the company Mr. Cravath was engaged on many important works; it would unnecessarily lengthen this memoir to go into detail, but it may be mentioned that whenever any survey or work of special difficulty was contemplated it was generally confided to Mr. Cravath, with the certainty that it would be well executed. The preparation of all the statistics and calculations in connection with the noted Sharpsville Railroad case, as well as all the surveys, were put in his charge; the printed reports attest his complete mastery of the subject in all its bearings, involving as it did some of the most intricate questions in the engineering and operation of railroads.

The difficult location of the railroad north of New Brighton, Pa., along the left bank of the Beaver River, was made by him in 1875, and when the line was built some eight or nine years later there was no reason for making any change and it was placed where he had located it.

His work with the Pennsylvania Company was such as involved a high order of intelligence and a thorough knowledge of the profession. All of his work was performed with that entire devotion to his employers' interests, and conspicuous disregard of self, which were characteristic of the man.

In February, 1880, Mr. Cravath left the service of the Pennsylvania Company to take the position of chief engineer of the Wisconsin & Minnesota Railroad Company; he located and built that road in about one year in the interest of the Wisconsin Central. A letter from the general manager of that company attests in eloquent and forcible words the value placed on his services and his personal worth. It says of him:

"In the performance of all his duties he evinced large experience, fidelity of purpose, and indefatigable industry; from being a professional acquaintance he came to be a warm personal friend. I grew to love him like a brother, and his sudden and untimely taking off was a shock from which we have not yet recovered. Cravath was an engineer of rare merit, a gentleman of culture and high principle, a friend in the truest, noblest sense of the word.

"It is well worth while to live such a life to leave behind such fragrant memories."

When Cravath had completed the Wisconsin & Minnesota Railroad he was appointed chief engineer of the Wisconsin & Michigan Road, a line running from Green Bay, Wis., two hundred and fifty miles, to Ontonagon, Mich., on Lake Superior.

The greater part of the surveys for this line were made by Cravath personally through a vast wilderness: braving the heat of summer, and exposing himself to the bitter cold of the severe winters; traveling on snow shoes in winter, and camping out where night overtook him, often alone, and sometimes lost in the trackless woods. Nowhere in his career has he better shown his indefatigable industry and indomitable will. No difficulty but spurred him to overcome it, no obstacle but inspired him with resources to surmount it. It was in the midst of active construction work during his engagement with this company, and largely owing to the hardships he imposed upon himself, that his health broke down, and he fell an easy prey to the Grim Reaper.

Roberth Cravath died of a congestive chill at his home in Green Bay, Wis., July 17, 1883, in his fifty-second year.

His services and his worth were not unappreciated: the vice president and managing officer of the company, himself an engineer and familiar from personal experience with the life and work of Mr. Cravath, writes:

"Mr. Cravath was our chief engineer for about three years and had the entire charge of the surveys and construction of our extensions and disbursed most of the money for the same.

"He was an engineer of more than ordinary ability, unquestioned integrity, and untiring industry; he never shrank from any exposure in the line of his duty; he won the respect and affection of all his associates here, and his sudden death is still mourned by them.

"I became very much attached to him, and thought I fully appreciated his services when living, but, when I look back and see what he accomplished, the exposure he endured, the courage and energy of the man at all times, I feel that these few lines are but a faint acknowledgment of his character and services."

The letters from which these extracts are taken speak for themselves; they proclaim the worth of the man and the value of the friend. We can not add more to the glowing tributes of his former associates.

His ambition led him to a faithful performnce of all his duties rather than to make any effort for notoriety. His natural modesty would not permit him to make any exhibition of the thorough knowledge he possessed of all branches of his profession, that of a railroad engineer.

None who knew him can forget his thorough kindness of heart. It led him to impart knowledge and experience almost without his seeming to know it. He won the respect and affection of all who came in contact with him, and longer acquaintance only added to the warm friendship he inspired in all who knew him well. Reserved in disposition, but strong in his attachments, he possessed a well-balanced mind and mature judgment. His modesty prevented him from making himself known to the many, and was so great as to hide much of his ability from the unthinking.

Cravath was one of the original members of our society, and though he never took a prominent part in our meeting, it is to such men, who

give their moral support, that the Engineers Society of Western Pennsylvania owes its present flourishing condition.

Since he is no more, let us mourn for the departed friend and associate, and let this memorial be a tender tribute, a wreath we lay down on the grave of Robert Cravath.

THOMAS RODD.
CHARLES ACKENHEIL.

EXPERIMENTS ON STEEL AND IRON RIVETED GIRDERS AND REMARKS ON THE TESTS MADE BY THE DUTCH GOVERNMENT.

BY C. L. STROBEL, C. E.

[A paper read before the Engineers' Society of Western Pennsylvania, Feb. 19, 1884.]

The *Railroad Gazette* of Jan. 26, 1883, contains an article by Chas. B. Bender, in which an epitome is given of the experiments made by the Government of Holland in the years 1878 and 1879, at the Harkort Bridge Works in Germany, on the comparative strength of steel and iron girders. The tests were made in connection with the construction of the Nymwegen Bridge, on full-size stringers and floor-beams, some of which had been intended for this bridge. The girders were tested by applying loads at the center, increasing them until failure took place. Thirty-two girders were thus experimented on, of which three were of wrought iron, and the remainder of steel of different grades as to hardness, the ultimate strength varying from 60,000 pounds to 120,000 pounds per square inch in the specimen.

The results of these experiments were so unfavorable for the steel girders, that the Dutch Government engineers, so it was stated, abandoned the use of this material for bridge construction in consequence. The ultimate strength realized out of eighteen "medium steel" riveted girders was actually less, in the average, than that obtained for the three iron girders, though the material in the former gave an average ultimate strength of 85,000 pounds per square inch in the specimen, against 55,000 pounds, the ultimate strength of the iron.

The publication of these experiments has not, so far as I know, been followed by a discontinuance of the use of steel in bridge construction outside of Holland. Indeed, in view of the scale upon which these experiments were conducted, it is surprising that so little attention appears to have been paid them, but the reason for this is to be found, I think, in their insufficiency to furnish a satisfactory explanation of the results obtained, which in themselves are improbable, and therefore require such explanation all the more. In the form in which they have been made public, the experiments are an enigma from which conclusions of any kind cannot be drawn.

In Mr. Bender's article want of uniformity in the material is advanced as the probable cause of the low strength of the steel girders. He supposes them to have given way by the failure, piecemeal, of the parts constituting each flange in the inverse order of their elasticity and ductility, and his report shows, that this actually occurred in a number of cases, ten of the girders exhibiting the remarkable phenomenon of breaking in one part of the tension flange (say in one angle) at a lower pressure than that which caused entire rupture. It will readily be admitted, that steel is a material so variable in its qualities, that such results *can* be obtained with it, but when we look for proof that such was the case for these girders, no evidence of this nature can be found. No tests are given on specimens cut from the material of the girders before they were strained, and none whatever are given for the angles. Such specimen tests as are contained in the report were made on pieces cut from the *plates* after the failure of the girders, and are not of a nature to convey the information sought.

If such great inequalities did occur in the material as would account for the low strength of the steel girders, the fault would appear to lie in the omission of the necessary precautions to secure proper material; but in that case, instead of condemning steel as unfit and abandoning its use as was done, it would appear more pertinent to pass censure on the methods of manufacture and inspection pursued which admitted of such results.

With the present low price of steel in this country, it can be a question of a short time only until its general adoption for construction purposes to fill the place now held by wrought-iron. Since 1879 three large bridges have been built at the works of the Keystone Bridge Company, the main truss members of which were made of steel, and I can say of this experience that it has been very favorable to the use of steel in bridge construction, and that no results have been obtained similar to those exhibited by the above girders.

Of a large number of steel eyebars tested to destruction none gave unsatisfactory results, and the ultimate strength, elastic limit and ductility of these bars showed great uniformity. These results could only, however, be obtained by careful physical tests of every cast of steel at the steel works before its use, the exclusion of all such casts as did not conform to the requirements, and the careful and appropriate manipulating of the steel in shop manufacture.

With these introductory remarks I wish to present the following contribution on the subject of the strength of riveted steel girders, in the form of a report of some tests on girders made on behalf of the Keystone Bridge Company, under my supervision. The materials were furnished free of charge for this purpose by Carnegie Brothers & Co., Ld., and the girders were manufactured and the tests made at the expense of the Keystone Bridge Company. The girders were strained in a similar manner as those of the Dutch Government.

DESCRIPTION OF GIRDERS.

There were five girders tested, intended to be exactly alike in form, but varying slightly in the thickness of metal of the constituent parts as indicated for each test. One of the girders was of wrought-iron, two of them were of ordinary Bessemer rail steel of .34 per cent. carbon, and two were

of mild Bessemer steel of .11 per cent. carbon. The girders were 12 feet long between centers of supports, and 12 feet 4 inches long, out to out; they were composed each of four 3 × 3-inch angles riveted to a 14 × ¼-inch web, with four angle stiffeners (two on each side of web) at center, and two angle stiffeners (one on each side of web) at ends. There was also a 4 × ½ × 6½ inch bearing plate at each end, and a 6 × ½ × 6 ½-inch plate at middle, riveted to flange angles. The rivets were of the same material as the girder, machine driven, ⅝-inch in diameter, spaced 3 inches center to center. One of the two hard steel girders and one of the two soft steel girders had rivet holes reamed to $\frac{7}{16}$-inch diameter from a $\frac{9}{16}$-inch punched hole, while the other girders had holes punched to the latter diameter without reaming. The iron was manufactured at the Union Iron Mills, Pittsburgh; the steel billets were made at the Edgar Thomson Steel Works and were rolled into the finished form at the Union Iron Mills. The web plates were rolled in the universal mill.

The steel was not specially selected for the purpose, and the .34 per cent. carbon steel would, on the contrary, not be considered suitable material for bridge construction, as its ultimate strength was about 100,000 pounds per square inch with elongation averaging 15 per cent., whereas the steel which has so far been used for tension members has an ultimate strength not exceeding 75,000 pounds per square inch with elongation of 18 per cent. The ultimate strength of the material in the soft steel girders was 62,000 pounds per square inch with elongation averaging 24 per cent.

The following shows the composition of the two kinds of steel from analyses furnished by the Edgar Thomson Steel Works:

	C.	Mn.	Si.	S.	P.
Soft steel	.11	.396	.019	.046	.088
Hard steel	.34	.954	.063	.113	.175

The following are the results of tests made on specimens cut from the material before the test of the girders. The specimens were 1 inch wide, and of the thickness of the plate or angle, and the elongation was measured in 8 inches:

IRON.

	Web Plate.	Flange Angle.
Elastic limit	not observed.	not observed.
Ultimate strength	51,000 lbs.	53,100 lbs.
Elongation	13 per cent.	15 per cent
Reduction	16 "	broke in grip.

HARD STEEL.

	Web Plate.	Web Plate.	Flange Angle.	Flange Angle.
Elastic limit	63,300 lbs.	63,000 lbs.	59,300 lbs.	60,800 lbs.
Ultimate strength	94,300 "	96,500 "	105,000 "	107,700 "
Elongation	14 per cent.	13 per cent.	17 per cent.	26 per cent.
Reduction	32 "	17 "	18 "	38 "

SOFT STEEL.

	Web Plate.	Web Plate.	Flange Angle.	Flange Angle.
Elastic limit........ ...	45,100 lbs.	49,700 lbs.	40,200 lbs	42,100 lbs.
Ultimate strength........	60,400 "	62,700 "	62,500 "	61,700 "
Elongation................	21 per cent.	21 per cent.	27 per cent.	28 per cent.
Reduction.................	57 "	55 "	58 "	60 "

In addition to the above, tests were made on specimens cut from the girders after they had been crippled under pressure. These specimens were of the same size as before. Two pieces were taken from the web near the neutral axis, and one from the unpunched leg of each angle of the compression flange, making four specimens for each girder, except for the iron girder for which two specimens were also taken from the angles of the tension flange and one of the web specimens showed a flaw and is not reported. The specimens were cut parallel with the grain. As the end of the specimen was about 15 inches from the end of girder, and they were 18 inches long, the center of specimen was 2 feet from end of girder, or 4 feet 2 inches from center of girder. The material in these specimens was therefore not strained up to the elastic limit in the girder, and yet in several instances the results of the tests are widely different from what had been obtained before the material was strained in the girder, and especially is this true of the iron. But as these tests have no direct bearing upon the subject, I present them without comment.

SPECIMENS FROM IRON GIRDER NO. 1.—(After test of Girder.)

	Web Plate.	Compress'n Flange Angle.	Compress'n Flange Angle.	Tension Flange Angle.	Tension Flange Angle.
Elastic limit................	30,300 lbs.	31,500 lbs.	31,800 lbs.	30,800 lbs.	31,100 lbs.
Ultimate strength..........	50,700 "	42,500 "	43,600 "	40,300 "	40,300 "
Elongation..................	12 per cent.	7 per cent.	7 per cent.	5 per cent.	5 per cent.
Reduction.........	14 "	14 "	15 "	11 "	16 "

SPECIMENS FROM HARD STEEL GIRDER, NO. 2.—(After test of Girder.)

	Web Plate.	Web Plate.	Compression Flange Angle.	Compression Flange Angle.
Elastic limit..............	38,600 lbs.	45,300 lbs.	55,700 lbs.	57,900 lbs.
Ultimate strength........	97,800 "	100,400 "	104,100 "	102,800 "
Elongation...	11 per cent.	13 per cent.	16 per cent.	16 per cent.
Reduction	28 "	30 "	34 "	39 "

SPECIMENS FROM HARD STEEL GIRDER, NO. 3.—(After test of Girder.)

	Web Plate.	Web Plate.	Compression Flange Angle.	Compression Flange Angle
Elastic limit	44,300 lbs.	48,500 lbs	51,100 lbs.	47,000 lbs.
Ultimate strength........	100,200 "	100,400 "	96,500 "	85,700 "
Elongation......	15 per cent.	15 per cent.	18 per cent.	25 per cent.
Reduction	42 "	39 "	40 "	44 "

SPECIMENS FROM SOFT STEEL GIRDER, NO. 4.—(After test of Girder.)

	Web Plate.	Web Plate.	Compression Flange Angle.	Compression Flange Angle.
Elastic limit	40,100 lbs.	41,100 lbs.	40,100 lbs.	40,300 lbs.
Ultimate strength	54,400 "	54,500 "	61,700 "	63,100 "
Elongation	29 per cent.	24 per cent.	23 per cent.	25 per cent.
Reduction	70 "	62 "	62 "	58 "

SPECIMENS FROM SOFT STEEL GIRDER, NO. 5.—(After test of Girder.)

	Web Plate.	Web Plate.	Compression Flange Angle.	Compression Flange Angle.
Elastic limit	42,500 lbs.	48,000 lbs.	40,900 lbs.	39,900 lbs.
Ultimate strength	61,900 "	62.500 "	63,500 "	60,600 "
Elongation	24 per cent.	24 per cent.	26 per cent.	26 per cent.
Reduction	66 "	62 "	62 "	61 "

TESTS OF GIRDERS.

The girders were tested in a vertical position in the large hydraulic testing machine of the Keystone Bridge Company. The pressure was applied centrally on the girders and was released after each reading. It was increased by increments of about 6,500 pounds. In the results given, no allowance is made for friction of machine, and they are therefore somewhat high, but the results for the different girders will compare well with each other. The time of test was approximately one hour for each girder. Each rivet was tried after the test and none found loose.

RESULTS OF TEST OF WROUGHT IRON GIRDER NO. 1.

Web, $14'' \times .27''$. Angles, all $3'' \times 3'' \times .31''$. Rivets, $\frac{5}{8}''$, holes *punched*, of .7″ diameter.

For solid section: area, 10.38 square inches; moment of inertia, 333.4.

For section considered reduced by rivet hole in bottom flange, top flange assumed solid: center of gravity .32″ above center of girder; moment of inertia = 315.2, assumed in calculation of fiber strain.

Center Pressure.	Deflection at Center in Direction of Pressure.	Side Deflection at center.	REMARKS.
Pounds.		Inch.	
13000	Measurement not reliable.	0	
19500		0	
26000		0	
32500		0	
39000		.01	
45500		.03	
52000		.03	
58500		.05	
63440	1.86 permanent.	.12 perm't.	Gradual yielding of girder, opening a rent in bottom flange.

Last center pressure corresponds to a strain in extreme fibers of 48,400 pounds per square inch in top flange, and 53,000 pounds per square inch in bottom flange. Girder gave way in tension flange. Break occurred in the projecting flanges of the angles, in line of rivets connecting stiffening angles near center of girder to web. Break started at the inner edges of the angles, next to web, working outward. Stiffening angles at this point were not split. Pressure dropped from 63,440 pounds to 52,-

000 pounds before it was released. The angles were reduced most in thickness at the inner edges where break started. They were about ¼″ thick at this point after fracture.

RESULTS OF TESTS OF HARD STEEL GIRDER NO. 2.—(Carbon .34 per cent.)

Web, 14″×.29″. Angles all 3″×3″×.33″ Rivets, ⅝″, holes punched, *not reamed*, of .7″ diameter.

For solid section: area, 11.59 square inches; moment of inertia, 353.7.

For section considered reduced by rivet hole in bottom flange, top flange assumed solid: center of gravity .32″ above center of girder; moment of inertia, 334.3, assumed in calculation of fiber strains.

Center Pressure.	Deflection at Center in Direction of Pressure.	Side Deflection at center.	REMARKS.
Pounds.		Inch.	
26000	Measurement not reliable.	0	
32500		0	
39000		0	
45500		0	
52000		0	
58500		0	
65000		0	
71500		0	
78000		0	
84500		0	
91000		0	
97500		0	Cracking sounds.
110500	2.06 permanent.	.10 perman't.	Gave way with loud report by fracture of lower flange.

Last center pressure corresponds to a strain in extreme fibers of 79,500 pounds per square inch in top flange, and 87,100 pounds per square inch in bottom flange.

Girder gave way in tension flange. Both angles were broken through and the web fractured at the edge. The break in one angle was through the middle rivet hole, and in the web and opposite angle through the rivet hole on each side respectively of the middle rivet. The middle rivet was sheared off clean and fell to the ground. Thickness of metal at break was not appreciably reduced. Fracture was clean and square, finely granular, showing slight furrows.

RESULTS OF TEST OF HARD STEEL GIRDER, NO. 3.—(Carbon .34 per cent.)

Web, 14″×.30″. Angles, 3″×3″×.34″. Rivets, ⅝″, holes punched to $\frac{9}{16}$″ diameter and *reamed* to .7″ diameter.

For solid section: area. 11.97 square inches; moment of inertia, 364.0.

For section considered reduced by rivet hole in bottom flange, top flange assumed solid; center of gravity .32″ above center of section; moment of inertia = 343.9, assumed in calculation of fiber strain.

Last center pressure corresponds to a strain in extreme fibers of 80,900 pounds per square inch in top flange, and 88,700 pounds per square inch in bottom flange.

Girder gave way in the top flange, which was bent S-shape, the versed sines for the two curves being 1½″ and 2½″ respectively. The lower flange was bent to a flat single curve with versed sine of ⅞″ occurring a short distance from the center. The top flange was buckled at the point of greatest side deflection.

Center Pressure.	Deflection at Center in Direction of Pressure.	SIDE DEFLECTION AT CENTER.		REMARKS.
		Total.	Perman't	
Pounds.		Inch.	Inch.	
13000	Measurement not reliable.	0	0	
26000		.07	0	
32500		.09	0	
39000		.10	0	
45500		.12	.02	
52000		.14	.06	
58500		.14	.06	
65000		.15	.07	
71500		.15	.07	
78000		.15	.07	
84500		.15	.09	
91000		.18	.14	
97500		.23	.19	
115700	1.85 permanent.	....	.49	Gave way quietly and slowly in top flange.

RESULTS OF TESTS OF SOFT STEEL GIRDER NO. 4.—(Carbon .11 per cent.)

Web, 14″×.26″. Angles, 3″×3″×.33″. Rivets, ⅝″, holes punched to $\frac{9}{16}$″ diameter and *reamed* to .7″ diameter.

For solid section: area = 11.17 square inches; moment of inertia = 346.9.

For section considered reduced by rivet hole in bottom flange, top flange assumed solid; center of gravity .32″ above center of girder; moment of inertia = 325.8, assumed in calculation of fiber strain.

Center Pressure.	DEFLECTION AT CENTER IN DIRECTION OF PRESSURE.		SIDE DEFLECTION AT CENTER.		REMARKS.
	Total.	Perm.	Total.	Perman't	
Pounds.	Inch.	Inch.	Inch.	Inch.	
13000	.08	0	.06	0	
19500	.12	0	.09	0	
26000	.18	0	.12	0	
32500	.22	0	.14	.01	
39000	.25	0	.14	.01	
45500	.30	0	.15	.02	
52000	.35	0	.17	.07	38400 compressive fiber strain. 42000 tensile fiber strain.
58500	.41	.07	.19	.14	
65000	.54	.17	....	.22	
71500	.61	.28	....	.26	
76700	....	3.08	....	.33	Gave way quietly and slowly in compression flange, pressure dropping to 52000 pounds.

Last center pressure corresponds to a strain in extreme fibers of 56,600 pounds per square inch in compression flange, and 62,000 pounds per square inch in tension flange.

Girder gave way in the compression flange, which was bent S-shape, the versed sine for the two curves being 1½″ and ⅞″ respectively. The tension flange was bent to a flat single curve with versed sine of ½″. The compression flange was strongly buckled near the center of girder and the web bulged and bent, conforming to the distortions of this flange.

RESULTS OF TESTS OF SOFT STEEL GIRDER NO. 5. (Carbon .11 per cent.)

Web, 14″×.28″. Angles, 3″×3″×.33″. Rivets, ⅝″, holes punched, *not reamed,* of .7″ diameter.

For solid section: area = 11.45 square inches; moment of inertia = 351.4.

For section considered reduced by rivet holes in bottom flange, top flange assumed solid: center of gravity .32″ above center of girder; moment of inertia = 332.2, assumed in calculation of fiber strain.

Center Pressure.	Deflection at center in direction of pressure.		Side deflection at center.		Remarks.
	Total.	Perm.	Total.	Perman't	
Pounds.	Inch.	Inch.	Inch.	Inch.	
13000	.04	0	0	0	
19500	.09	0	.02	0	
26000	.14	0	.04	0	
32500	.18	0	.05	0	
39000	.22	0	.08	.04	
45500	.27	0	.09	.04	
52000	.32	0	.09	.06	37700 compressive fiber strain. 41300 tensile fiber strain.
58500	,37	.03	.09	.07	
65000	.46	.06	.12	.08	
71500	.55	.16	.13	.11	
81900	....	2.20	....	.11	Gave way in tension flange.

Last center pressure corresponds to a strain in extreme fibers of 59,300 pounds per square inch in compression flange, and 65,000 pounds per square inch in tension flange.

Both tension flange angles were torn asunder; one across rivet in stiffening angle, and the other across adjoining rivet on the side towards the nearest end of girder. The compression flange began to buckle slightly next to center bearing plate and there were indications of strain lines on web at center next to stiffening angles. These strain lines were most strongly marked next to the compression angles.

CONCLUSIONS.

1. Each of the steel girders showed a large increase in strength over the iron girder; the soft steel girders proved 22 per cent. stronger and the hard steel girders 66 per cent. stronger than the iron girder.

2. The greater strength of the soft steel over the iron in the specimens was fully attained, and exceeded, in the girders.

3. The hard steel girders did not show so large a percentage of greater strength over the iron girder as did this material in the specimen over the iron in the specimen. This may be accounted for as the result of punching the rivet holes without reaming, for it was the girder with punched holes which gave way by the fracture of the tension flange, whereas the girder with reamed holes gave way in the compression flange, and probably would have stood more before fracture had taken place in the tension flange. The latter girder did not appear to bear truly upon its supports, and it was probably this which caused it to fail in the top flange when it did.

4. Punching rivet holes without reaming did not produce any results other than an apparent loss of strength as compared with reamed holes.

5. The strength of steel girders strained in the manner of these girders appears to be about the same for the two flanges if they are made alike in section.

A NOTE ON ALLEGHENY RIVER WATER.

BY PROFESSOR FRANCIS C. PHILLIPS.

[A paper read before the Engineers' Society of Western Pennsylvania, Feb. 19, 1884.

In proportion as any district becomes more and more densely populated, no question in sanitary affairs assumes greater importance than that of the selection of a reasonably safe source of water supply.

But as the main river courses are, as a rule, called upon to perform the double office of supplying the great cities with drinking water and serving as a national sewerage system, it has been found necessary in every community from time to time to subject the river waters to close investigation, in order to ascertain to what extent the two functions of the river are compatible. With shorter river courses and more closely built cities the question has naturally arisen more frequently in England than in America, and the most elaborate and thorough researches have there been instituted with a view to establishing criteria by which to ascertain the freedom of a river water from the dangerous elements of sewage and the refuse of manufactories. The first results of such study have shown that the conclusion to be reached must not be influenced by mere observation or preconceived opinion, but must be the outcome of laborious investigation. Originally, and apparently with good reason, the fullest hopes were placed in a chemical solution. A water, which on evaporation, left a residue which showed signs of charring when heated and emitted an offensive smell was on that account condemned. This was very well so far as it went, but many cases are reported where a water free from such quality was known to be the cause of the spread of typhoid and other diseases, while in other cases a water convicted on such evidence has proved itself to be perfectly wholesome.

It is a decree of nature that of the elements a certain few shall never be at rest, but whether in living or dead matter, shall for all time change continually their mode of grouping. No sooner does the life depart than these groups are broken up by the process of putrefaction into simpler and simpler groups, until the final products become wholesome food for a new generation of living beings. But there is no more mysterious fact in biological study than this: that these intermediate stages of matter between the living organism and the final products of decay—ammonia, water, carbonic acid, are so terribly destructive to life.

It was a step towards a rational system when methods were invented based upon the studied changes occurring in putrefaction.

Knowing that the minute quantity of indefinable and intangible sewage matter will, in the river or well water gradually generate a corresponding quantity of ammonia, we hasten by chemical means, this change, and then by an exceedingly delicate test determine the proportion of ammonia.

This ammonia is a measure of the organic impurity, and is known commonly as "albuminoid ammonia." By an equally delicate method the quantity of oxygen can be measured which is just sufficient to oxidize or burn into carbonic acid the carbon of the organic matter, just as we might roughly attempt to measure the weight of a quantity of coal by the quantity of air needed to burn it in a furnace.

Water which yields a large proportion of ammonia is suspected to contain an excessive amount of nitrogenous bodies, while if it requires a relatively large amount of oxygen to burn the organic matter held in solution, this fact is regarded as further evidence of contamination.

It has been supposed that the presence of nitrogenous bodies indicated matters of animal origin, while those of highly carbonaceous character are evidence of vegetable impurity.

Hence greater importance has naturally been attached to the ammonia process, so-called, as being an indicator of the more dangerous constituents of impure water. As no such thing exists in nature as absolutely pure water, it becomes necessarily a question as to the relative impurity estimated by comparison with some other water assumed as a standard or with the average of good waters.

According to the dictum of Wauklyn, which has been generally accepted by authors, a water containing much more than ten parts in 100,-000,000 is to be considered suspicious. Wauklyn's estimate seems to be based upon the results of repeated tests of London drinking water.

It has been objected, however, that chemical methods do not distinguish between the dangerous and the harmless impurities of organic character, and that no account is taken of those mysterious little organisms called bacteria so frequently charged with being the cause of disease. From 32 to 38 different species of microscopic organisms were detected by Dr. Lankester in a half gallon of London water, and yet the influence of such organisms upon the safety of the water could not be defined.

A commission has recently been appointed by the National Board of Health in Washington, to examine into existing methods of water analysis. The report of the commission covers a wide range of inquiry, and the results have done much to place this difficult subject in a clearer light, by showing that the analytical data are chiefly of value. 1. In the case of seriously polluted waters; and, 2. in the periodical tests of drinking water for ascertaining changes in its purity.

My allusions to the chemistry of the subject, while they are superfluous to those accustomed to deal with such matters, may serve as a preliminary to a few notes of some examinations of Allegheny River water which I have carried out for some time past.

The purpose of the work was to gain some insight into the daily fluctuations of the impurities and their relation to the height of the water.

It seemed desirable to secure a record of results obtained during a period of diminishing depth and high temperature, and also at a time of increasing depth and low temperature.

The tests were made: 1. During May and June; and, 2. From September 24th to December 21st, 1883. The water was drawn from a hydrant on North avenue, Allegheny, between 2 and 4 o'clock in the afternoon of each day.

The results are represented graphically in the accompanying table.

The upper curve gives the depth in feet of the Allegheny measured at the Suspension bridge, for each day of the period covered by the tests. The dotted part of the curve indicates that the water was at or below the lowest mark on the scale. As it turned out the approach of hot weather did not bring with it a low stage of water.

The lower curve gives the "Albumenoid Ammonia" in parts per 100,000,000. In the lowest part of the table is given the rainfall at Pittsburgh, in tenths inches, as recorded by the Signal Service officers.

It is noticeable that when on May 8th, the river fell below the lowest mark on the bridge pier the ammonia diminished to six parts in 100,000,000. The rise of the river on May 13th, 14th and 15th, seems to have scarcely affected the ammonia, but the rise to 15 feet on the 22d corresponds to an increase in the ammonia to 18 parts per 100,000,000. In general there is, I think, a sufficient resemblance between the curves to warrant the conclusion that the ammonia, or in other words, the nitrogenous impurities, vary in amount with the depth of the river.

In the Autumn the resemblance between the two curves is less apparent, but a general coincidence in their direction is observable throughout, notwithstanding that at certain periods they seem to diverge. From October 9th to October 30th the river was below bench mark and changes of depth were not measurable. The maxima in the ammonia curve are seen not to coincide with periods of greatest local rainfall. The largest amount found, on December 13th, corresponds to a rainfall of only 0.03 inch.

As to the quantity of organic matter, it may be said that only the highest points of the curve indicate a proportion sufficient to call in question the wholesomeness of the water.

The region drained by the Allegheny River, covering the greater part of Western Pennsylvania and the southwestern counties of New York State has an area of 10,764 square miles. The population of the entire watershed according to the census for 1880 is 567,000. This represents an average of 52.6 persons to each square mile of territory drained by the river and its tributaries, excepting from the estimate Pittsburgh and Allegheny. The following table taken from the report of the Water Commissioner of the City of Troy, 1877, gives the relative density of population per square mile of watershed for some of the chief cities (excepting Philadelphia and Pittsburgh):

POPULATION OF WATERSHEDS FOR WATER SUPPLIES OF CITIES.

	Population per square Mile.
Rochester, N. Y.	36
Pittsburgh	52.6
New York, N. Y.	65

	Population per Square Mile.
Albany	77
Poughkeepsie	86
Schenectady, Cohoes and West Troy	103
Brooklyn, N. Y.	119
Philadelphia	200
Boston	229
London, England	270

Inasmuch as very few of the smaller towns and villages in Western Pennsylvania are sewered, it is evident that the superiority of Allegheny water to that supplied to large Eastern cities must be greater than appears from a comparison of the figures in this table.

Flowing through a region so sparsely populated the purity of the water before it reaches Pittsburgh cannot be doubted, and as the Pittsburgh water works are above the city there is no danger of sewage contamination. The case of Allegheny City supply is somewhat different. The Thirteenth Ward sends a small portion of its surface water, together with the drainage from a limited number of houses into the river three-fifths of a mile above the water works. But there are seven sewers on the Pittsburgh side above the lower end of Herr's Island. These sewers drain a large portion of Pittsburgh having a population of more than 40,000. As to the proportion of this sewage likely to be mingled with the river water on the Allegheny side an estimate is of course impossible. The close proximity of large tanneries in operation and in process of construction is not an agreeable feature, and should suggest the importance of steps towards protecting the water from such dangers.

The organic matters of the water appear to increase with the quantity of water flowing through the channel. Were these impurities due to local causes we should expect to find their quantity greatest when the water is low, that is when the quantity of sewage is greater in proportion to the quantity of water in the channel, and least when the river is diluted by heavy rains.

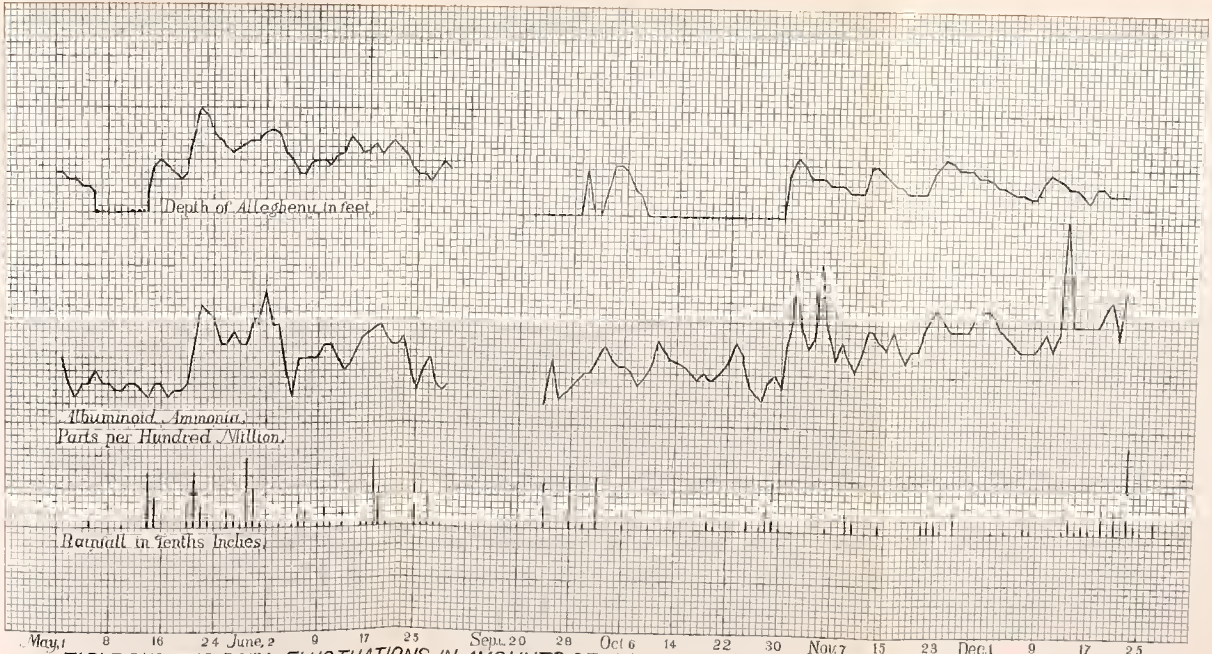
As the maximum impurity coincides with periods of high water, it seems reasonable to conclude that the organic matters are traceable to the general drainage of the soil throughout the sparsely settled watershed of the Allegheny.

When the water is very muddy the proportion of nitrogenous matters is very much greater in the sediment than in the filtered water. During the recent flood 70 parts per 100,000,000 of albumenoid ammonia were found in the muddy deposit which collected at the bottom of a half gallon jar of water on standing 24 hours. This is highly suggestive of the necessity for a settling reservoir. If an opinion can be based upon a consideration of these tests, it seems probable that Allegheny City water with the exception of periods of maximum impurity not easily anticipated or explained, possesses a degree of purity entitling it to be considered safe.

The average result of 77 tests of Pittsburgh hydrant water made during October, November and December, 1881, was 11.1 parts of albumenoid ammonia per 100,000,000.

The average for the same period of the year for 1883, in the case of Allegheny City water was 12.2 parts per 100,000,000.

The question of the sanitary influence of the water may remain at rest



1888 TABLE SHOWING DAILY FLUCTUATIONS IN AMOUNTS OF NITROGENOUS MATTER IN ALLEGHENY RIVER WATER.

perhaps for some time to come. Yet the difficulty of foreseeing the real necessities of the future water supply are well illustrated by reference to the history of Cincinnati, where 66 years ago a water company was chartered to supply the city from a wooden tank. For this privilege, which was exclusive and was to continue 99 years, the company was to pay $100 a year, and was required perpetually to carry its pipes three feet higher than the kitchen floor of a prominent citizen.

The value of systematic water tests would be greatly increased could accurate medical statistics be simultaneously collected of all diseases even remotely traceable to impure water. Only fatal cases are usually recorded.

If, in addition to their exact study of the atmosphere, the Signal Service officers could conduct periodical tests of the water supplied to the great cities, valuable results might be anticipated, which would unquestionably tend to solve many difficult problems in the difficult study of the relationship of water supply to public health.

IS THE DESTRUCTION OF FORESTS A CAUSE FOR THE INCREASE IN THE FREQUENCY AND HEIGHT OF FLOODS?

BY THOMAS ~~T~~ P. ROBERTS, C. E.

[A paper read before the Engineers' Society of Western Pennsylvania, March 18, 1884.]

The terribly disastrous floods in the Ohio Valley two years in succession have naturally created a wide-spread feeling of alarm. Not only have the river bottoms between Pittsburgh and Cairo been overflowed by one or the other of these floods, to a greater depth than was ever before known, involving the total destruction of over 1,000 buildings, and the loss to farmers of hundreds of thousands of dollars by the destruction of fences and produce; but the business streets of such cities as Pittsburgh, Wheeling, Cincinnati and Louisville, have been invaded by the turbid torrent, which found its way into stores and warehouses filled with valuable goods heretofore considered safe from such dangers. It is too early to calculate the aggregate loss caused by these floods, though it is known the gross sum will be millions of dollars. Men stand aghast facing destitution in many places, and fear to rebuild their homes or plant crops, having a foreboding that if they so do their labors will be for naught, should the waters come up again. In Cincinnati the rise of 1883 was higher than any previously recorded flood, but the great wave of 1884, 71 feet 1 inch high, overcapped even that by four and a half feet.

The question is anxiously asked, is there a limit to the height which the record of these floods will climb? Or can science say more confidently than did the flattered but not beguiled Canute to the rising tide on the English coast, "Thus far shalt thou go and no farther."

We fear the answer to the question must be "No!" So far in its history meteorology is a science of observation and record. We may indeed collate the result of many observations, but so-called predictions concerning the future of the weather, beyond at the utmost three or four days, have their sole basis on the system of means—"Poor Richard's Almanac" and Vennor exemplify this method. We believe, in other words, that what Nature has done before is apt to be repeated, and because the annual pre-

cipitation of rain and snow in Western Pennsylvania, during several decades has been 34 inches, that it will maintain this, or near this average indefinitely in the future. But there is little comfort in this knowledge, as when we know that in one year it may rain only 20 inches and in the next 48 inches without disturbing the mean figure 34 inches.

The flood sufferers in the Ohio Valley have another question to ask which may possibly be susceptible of an answer, and that is: "Has human agency anything to do with the increase of the rainfall; or if the rainfall be the same, has, to state it more concisely, the clearing of forests, the draining of swamps, and the tillage of the soil, by expediting the discharge of the rain into brooks, and creeks, brought to the rivers more water than Nature intended them to pass during given intervals?"

Here is a definite question, and as we have various records of river floods during the past, from the time, particularly in this country, when forests were scarcely touched by the hands of man, down to the present, it strikes us that it can be intelligently discussed. All those who have given the question any study have doubtless observed a wide divergence of theories on the subject, developed from the same data, and very frequently met with wordy wise opinions *pro* and *con*, having but little basis of facts. A man may go into a virgin forest region put up a small saw-mill on a little brook, which for several years supplies power to his mill for ten months out of the year, bye-and-bye the land becomes cleared about him and the springs which supply his brook diminish their yield, and then his mill can run only two months in the year. This is fact No. 1, and the saw-mill man builds upon it. "Rivers," he says, "are formed by the united tributes of many brooks. Thousands of others besides myself have been cutting down the timber and girdling the trees, all the brooks consequently must be just like mine." Presto! "As the whole is equal to the sum of its parts, the rivers of his region must be dried up ten months of the year." The next we hear of this man is through the columns of the Hayville *Weekly Herald*, where he startles his readers with the boldness and novelty of his assertions. No person can dispute with him, and he carries everything, because it is universally conceded that the basis of his argument is sound. Meantime at the rivers, steamers are coming and going, Captain Gangway notes in his log-book, "average water of last few years, better than the past, made more trips, business prospering, all we need from Congress is an appropriation to clear out a few rocks." Does he not know that Hay Creek is dried up? No. He never heard of Hay Creek. Has Mr. Sage of Hayville, ever looked at Captain Gangway's river record? No. He never heard of it, and besides if he were to see it he would prove that it was of no value, because "what can a poor river man know of cycles of time," and mathematics generally? "What can such a man, who is not a subscriber to the agricultural reviews know of evaporation, rainfall, and the necessity of tree culture."

So the question is greatly vexed with the science all on one side.

GERMAN RIVER RECORDS.

The problems involved in the determination of this question cover such a wide range that it would be impossible in the time allotted to speakers at our meetings to more than glance over the field.

My chief object will be to lay before the society such data as I have been able to obtain bearing on this subject in the hope that the references may save some labor to those who may desire to glean the field.

English readers are indebted to General Godfrey Weitzel of the Corps of Engineers United States Army, for a complete translation from the German of the "Treatise on the Decrease of Water in Springs, Creeks, and Rivers contemporaneously with an Increase in Height of Floods in Cultivated Countries," by Gustave Wex, Chief Engineer of the Improvement of the Danube at Vienna. I mention this paper particularly as it is an epitome of facts and figures, tables and plates, of great value to Americans who wish to know how this question is discussed in Europe. I regret that it is not in my power to exhibit all his diagrams, which form as he thinks, the solid basis for his sweeping conclusions.

I can only briefly refer to some of them. On sheet No. 1, he exhibits a plot illustrating the highest, lowest, and the calculated mean annual stages of the Rhine at Emerich from 1770 to 1835. I have illustrated on my diagrams, however, only the flood lines. He divides the 66 years into two periods of 33 years each, and he then finds that the mean height of floods in the second period of 33 years has increased .086 of a foot, or about one inch. It is quite unfortunate for his theory in this instance that the individual highest flood, viz., that of 1799, when the river rose to the 25 foot mark was in the first period. The general increase of the flood height by one inch in the second period leaves nothing settled. Mr. Wex confines most of his remarks in the text to the mean and lowest stages, and in his Rhine record, as in fact in all his records, finds more grounds for the support of his theory of less mean annual discharge than he can for his assertion that the floods are increasing. It is not my purpose to enter into these details.

The questions of greatest importance in this country are: Are floods increasing in frequency and height? We may let the low water stage decline as it will, provided we are sure that the climate is not changing, and that floods are not increasing on account of the clearing of forests, and the cultivation of the soil. A thorough scientific investigation of the subject must necessarily enter into *every* detail, and it is true, from the low water records alone, can any reliable results be reached, touching changes of seasons and rainfall, but in this paper this question can be only briefly alluded to. Mr. Wex shows that the mean low water depth at Emerich, in the second period of 33 years has declined 1.16 feet. This would indeed be startling if we could be assured not only that the gauge marks were stationary during the entire period of 66 years, but also that the works of river improvement and continued navigation has not tended to scour out the river bed. This is the common experience; gauge marks set in a pool above a bar or ripple which has either been dredged out or washed out by dikes concentrating currents upon them, soon have their zeros out of water, so that calculation of the real low water discharge of water based on their records will give exceedingly erroneous results. As M. Pasqueau states,* and speaking of 19 diked passages on the Rhone: "At all, without exception,

*Page 88 "Improvement of Non-Tidal Rivers," Translations of Colonel Wm. E. Merrill, United States Engineers.

the water level has been considerably lowered, the neighboring ripples have been aggravated, etc." Mr. Wex had his attention directed to statements similar to those of M. Pasqueau, made by other distinguished authorities, and it is to be borne in mind that the Rhine, the Elbe, the Oder, the Vistula, and the Danube Rivers, which he treats upon, were, during the later periods covered by his records, all being improved by dikes, etc. It is conceded by geologists that the beds of all rivers, unless they are of more than adamantine firmness, are being gradually but perceptibly lowered, and of course the low water surfaces go down with them.

At one period, no doubt, the hills about Pittsburgh, now over 400 feet high, were less than 50 feet above low water surface, a sixteenth of an inch per year of scour in the river bed would be plainly perceptible on a gauge standing for 66 years, as it would amount to 4⅛ inches. But if we were assured that each daily low water discharge of the Rhine had been separately calculated from original data no one could dispute the accuracy of Mr. Wex's statements, but from the text, readers are left to infer this was only occasionally done. Entire reliance on standard gauge readings, with computations by fixed formula have been most probably the methods employed by him on all the rivers.

OBSERVATIONS ON THE ELBE AT MAGDEBURG.

Mr. Wex presents a diagram similar to the one described of the Rhine at Emerich, giving high, low, and mean annual stages of the Elbe at Magdeburg from 1728 to 1869. He divides this long period of 141 years into three nearly equal divisions. During the first period, 1728 to 1778, the high water averaged 16.159 feet, the greatest flood occurred in 1775, viz.: 17½ feet, in all twelve floods are recorded above the 17-foot line.

The second period embraced the years 1778 to 1827. During this time the average height of floods as compared with the former periods was *lowered* 1.242 feet. The highest flood rose to 18.3 feet, there being in all fifteen floods above the 17-foot line.

During the third period, viz.: 1827 to 1869, the average flood plane rose 0.309 feet, making it 15′ 4.33″, as compared with 16′ 1.59″ of the first period. The water rose to 19 feet in 1846, and during the period was six times over the 18-foot mark, and in all was thirteen times over the 17-foot mark. The low water surface is recorded to have fallen from the first period, the mean of which was 5.693 to 3.936 in the second period, and to 3.107 feet the mean of the last period, a total declination of over 2½ feet. Mr. Wex devotes several pages to the discussion of the results, which I have briefly condensed above; but again it is to be remarked that his decreasing table of low water depths is not supported by calculations of discharge. Reference is made to the reports of various commissions of engineers, but they nowhere in terms distinctly assert that the low water volume of the Elbe is gradually but surely diminishing. Mr. Wex is almost alone with his theory. The Commission of 1842 did, however, express the following views in their report.

*"That, although we have no material on hand to enable us to treat this subject in an exhaustive manner by the comparison of figures, yet it is

*Wex, page 17.

proper to state that a marked recent decrease in the discharge of the stream *is not* indicated by experience on observation," but then they add, as if apparently having heard some skillful arguments: "But we recognize the fact that the clearing of forests, cultivation of marshes, and irrigation of lands are able to produce a marked decrease in the quantity of water in the stream." A later commission agreed that the water of 1842 was lower than any previously known, and that the decline of 1857 and 1869 went even below the mark of 1842 at several points near the headwaters; but strangely enough the low water mark of 1842 at Magdeburg, the prime point of observation, has not been bared since that date.

As far as experience on the Elbe is concerned, it appears to have been proved by Mr. Wex that floods in this century have twice exceeded the highest waters recorded previously, viz: in 1845, 9 inches, and in 1862, 6 inches higher than the flood of 1785. This is the result of the study of 141 years' record, and is of little practical moment—at least it should create little alarm. During the first half of this long period twenty-two floods reached to or exceeded 17 feet rise, and during the second half, twenty-three floods reached to or exceeded the same mark. Nature's division, it strikes me, was remarkably even.

OBSERVATIONS ON THE VISTULA AT KURZEBRACK.

In 1858, or three years after the disastrous flood of 1855 on the Vistula, Mr. Schmid, Royal Prussian Privy Counsellor, published detailed records of the fluctuations of the Vistula at Kurzebrack during a period of 48 years —from 1809 to 1856 inclusive, and gives the following opinion:

*"That the apprehensions which were created in the country by the extraordinary flood of 1855, that larger quantities of water flow into the Vistula during recent than in former years are not well grounded, and that the tables rather indicate that a decrease in the quantities and stages of the water may have taken place."

Mr. Wex then takes up the subject and supplements the tables by the records to 1878. He then divides the 63 years into two nearly equal periods. Taking his own diagram we discover that in the first period of 32 years the river on nine occasions rose above the 20-foot mark, and that once in 1829 it reached 24 feet. In the second period it rose also nine times above the 20-foot mark, but at no time did it reach 24 feet, except during the extraordinary flood of 1855, when it rose to 28.5 feet, but even with this unusual rise included, the mean of high water in the second period is not by one inch as high as the mean of the first period.

But Mr. Wex differs with Mr. Schmid, for he says: "A much larger quantity of water flows into the Vistula, and these now produce more numerous, higher, and consequently more destructive floods than in former times." Mr. Wex probably does not desire to be considered an alarmist, but I think he reached this conclusion from a misapprehension of his own records.

THE DANUBE AT VIENNA.

Mr. Wex presents the records of the Danube at Vienna from 1826 to

*Wex, page 19.

1874 inclusive. Dividing this into two equal periods, I find that during the first, ten floods exceeded 8 feet, not including several produced by ice gorges. The highest rose to 9′ 3″, and the mean of the high water was 7′ 9.37″.

During the second period only six floods exceeded 8 feet, but two of them were exceptionably high, one rising to 12′ 2″ and the other to 11 feet, notwithstanding which the mean height of floods for this period was over 9 inches lower than for the first period.

Ice gorges are, Mr. Wex says, very frequent on the unimproved portions of the Danube above Vienna, and these exceptional floods, one of which occurred on Feb. 5, 1862, and the other Jan. 21, were possibly augmented by them. A gorge breaks sometimes and acts like a broken dam in creating a pulsation which every gauge below will record merely as a passing flood. In searching the record of the Vienna floods, I find that of the fourteen *summer floods* during the 1826–42 period, when rains would have most to do with them, the highest rise was 9′ 3″, and that three of the fourteen exceeded 9 feet. During the second period there were sixteen summer floods, one of them being 9′ 5″, and that none of the other fifteen floods reached so much as 9 feet."

On page 24 Mr. Wex says: "The seven hydraulic experts who were invited by the commission on the improvement of the Danube to give an opinion on this matter did not consider that the decrease in the discharge of the Danube at Vienna mentioned by me as yet a clearly proven fact."

If the seven could not go with him on his low water deductions, which have a plausible basis at least, for argument, we cannot doubt they would have decidedly opposed his assertions that the floods are increasing either on the Daunbe or any other of the rivers he treats of.

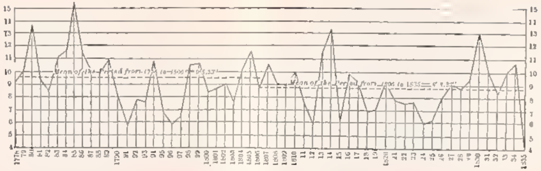
THE DANUBE AT ORSOVA.

Mr. Wex's diagram of the floods of the Danube at Orsova is very unfortunate for his theories. The period embraced is between the years 1840 and 1871. Divided into two equal periods, it is shown that in the first there were eight floods exceeding 15 feet, two of which rose to 19 feet. In the second period, there were also eight floods exceeding 15 feet, but none of them exceeded 17 feet 9 inches. The mean of flood heights was, in the second period, 1 foot lower than in the first period.

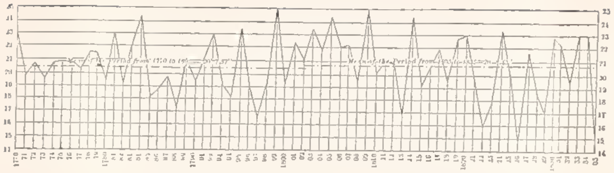
THE ODER AT KUSTRIN.

Lastly, Mr. Wex tabulates the record of the Oder river, at Kustrin, but does not speak at length upon it. The annual records extend from 1778 to 1835, divided into two equal periods of 29 years each. During the first period 11 floods exceeded 10 feet rise, one of which was 13.8 feet, and another 15.6 feet. During the second period there were only six floods exceeding 10 feet, and the highest rose to only 13.4 feet. Mr. Wex certainly took no mean advantage of silence, nor did he commit the sin of "omission" in this case. On the whole, it can be confidently affirmed that the records of many years on five of the most important rivers of Europe indicate that there is no evidence to support the theory that floods are

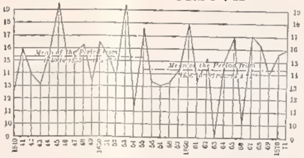
ODER AT KUSTRIN



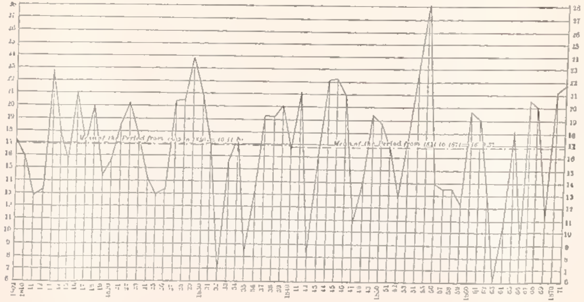
RHINE AT EMMERICH



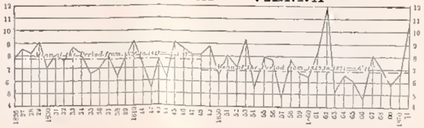
DANUBE AT ORSOVA



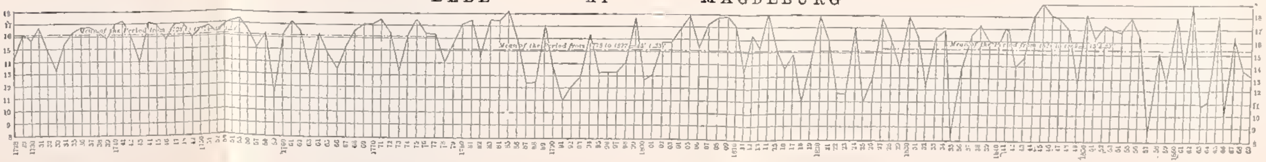
VISTULA AT KURZEBRACK



DANUBE AT VIENNA

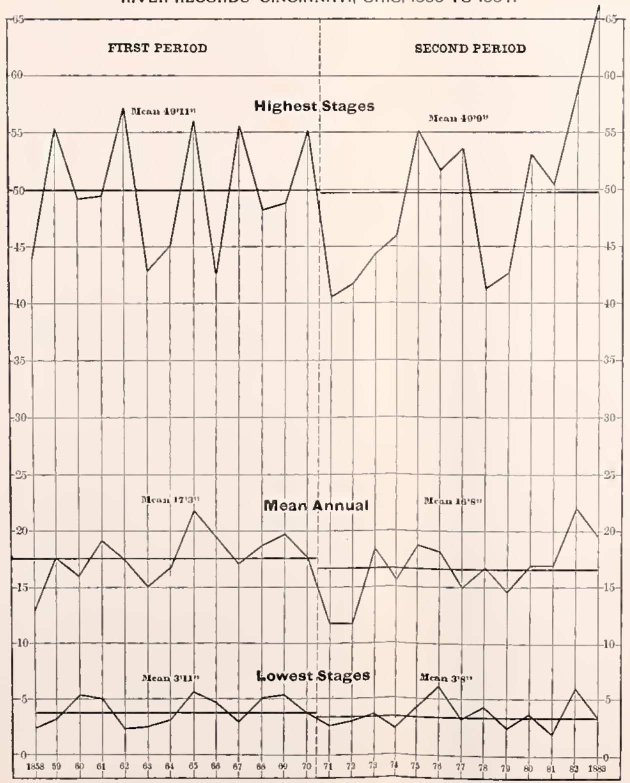


ELBE AT MAGDEBURG

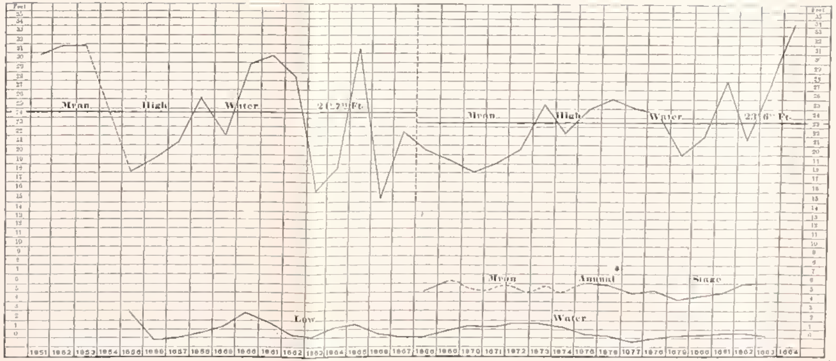


IS THE DESTRUCTION OF FORESTS A CAUSE FOR THE INCREASE IN THE FREQUENCY AND HEIGHT OF FLOODS?

RIVER RECORDS—CINCINNATI, OHIO, 1858 TO 1884.



RIVER RECORDS—PITTSBURGH.



IS THE DESTRUCTION OF FORESTS A CAUSE FOR THE INCREASE IN THE FREQUENCY AND HEIGHT OF FLOODS?

increasing in height or frequency, and as to whether the destruction of forests causes a less annual low water or mean annual discharge is an open question.

CLIMATIC CHANGES.

As it may be interesting to note the grounds on which Mr. Wex and others base their theories, that the destruction of forests affects the high and low water stages of rivers, I will here introduce some paragraphs from various authorities:

* "Dr. Berghaus mentions, in his Hydrography, heretofore cited by me, that the decrease in the discharge of the Elbe since 1782 is caused by the decrease of the rain-fall which has taken place in its basin since that time. The cause of this is stated, both by Berghaus and Malte Brun, to be in the clearing of forests, since the attraction of the latter draws electricity and water from the clouds and increases the amount of the rainfall."

It is a common source of wonder with Germans in this country that our forests are not protected and renewed as they are in Germany, but if we can believe Berghaus and Malte Brun, the German laws on this subject cannot be universally applied throughout the empire.

†"It is a fixed fact which has been proven by numerous observations that the clearing of forests dries up springs, and that when they again grow the latter flows more abundantly and regularly. * * * *" Boussingault says, "It is my opinion that the clearing of forests over a large area has always the effect of diminishing the annual rain-fall." The learned Coultas makes the following comparison: "The ocean winds and forests may be considered as the different parts of a great distillery. The sea is the boiler in which the steam is created by heat of the sun. The winds are the pipes which lead the steam to the forests where a lower temperature exists. For this reason the steam is condensed, and in this manner forests distill showers of rain," etc.

The "opinion" of a rain-guage before and after the clearing of a particular forest region would have been an admirable endorsement of Boussingault's views.

‡ Mildrum, the director of the observatory on Mauritius island, has found that since 1852, when at least 44,155 acres had been cleared, the rain fall, humidity and pressure of vapor had decreased, and on the contrary, the floods and dry periods have increased.

The clearing of less than 70 square miles producing such decided effects is remarkable. Seventy square miles represents about the one-ninth of the island, but such areas have frequently been cleared in the United States without in the least decreasing the rain-fall, as I hope to show hereafter.

§ "The report of Mr. Matthieu, professor of the Imperial School of Forestry, was published by the French government, in which he shows that it is proven by experiment that the amount of aqueous vapor over a cleared field is five times as great as over ground covered with forests, which he considers equivalent to an increase in the annual rain-fall in sections of countries covered with forests. Professor H. W. Dove, one of the

* See Wex, page 32. † Wex, page 33. ‡ Wex, page 33. § Id page 33.

first authorities in the knowledge of weather, says: "Europe has worked itself into continually irregular rainy seasons by the modern cultivation of its lands, which crowds off the forests recklessly, and which causes its rivers to be nearly dry during long periods, while during others their banks can hardly retain the masses of water which are poured into them." Then Mr. Wex, in his summary, says:

" The existence of forests in a country increases the amount of rainfall, because the fogs and clouds, saturated with aqueous vapor, which sweep over it, are, in the first place, condensed by contact with them and fall in the shape of rain." This short, simple assertion could have been verified in one year's time by observations on neighboring rain gauges—one in a forest and the other stationed in an open plain—but Mr. Wex quotes no one who has tried this experiment. He continues to say:

" Forests increase the amount of subterranean seepage and water in springs considerably, since the rains, being retained by the leaves of the trees, fall to the earth slowly. They are then retarded in their flow by the spongy surface, and are partly soaked in, and partly sink into the deeper layers of the earth, etc.

Professor G. K. Gilbert, in Powell's Land of the Arid Regions, in a report on the water supply of the Great Salt Lake, of Utah, says:

" The cutting of trees for timber and fence material and fuel has further increased the streams. By the removal of foliage, that share of the rain and snow which was formerly caught by it and thence evaporated, is now permitted to reach the ground, and some part of it is contributed to the streams." Thus authorities will philosophize differently.

Again Mr. Wex says: "A further cause of the decrease in the discharge of springs and streams of many countries in Europe, during the last decades, is the emptying of lakes and ponds, and the drainage of swamps and marshes." We will let Professor Gilbert answer this. He says, quoting from the report just mentioned: "Small springs are apt to produce bogs, from which much water is evaporated," etc., etc. It appears arguments can be produced from nature to support any theory. She rains not only on the just and the unjust alike, but quite possibly denies the plowed field, which needs it most, no favors which she would give the woodlands.

Lastly, Mr. Wex says, that in cultivated and thickly settled regions, much water from our rivers is withdrawn for the purposes of irrigation—he might have added, for manufactures, also. It is believed that scarcely a half of the water used by great cities returns to the streams from which it was originally drawn. Undoubtedly such considerations as these would have great weight in some regions dependent upon the proportion withdrawn for these purposes as compared with the original stream. There is little irrigation practiced in the Ohio basin, and the quantity of water pumped out to supply the cities lining its banks, is probably too insignificant a proportion of the whole low water discharge to require notice.

The *Sanitary Engineer*, in a recent article, says: "The surface of the ground, when shaded by forest trees, is always covered with a spongy layer of old leaves or mosses favoring the absorption of water, while the fallen logs and underbrush obstruct the flow on the surface. After the

clearing of the forests the mosses dry up and the old leaves soon decay; so that it freezes more readily and favors the rapid drainage of surface water."

We have never had but one destructive summer or fall flood on the Ohio, and that was in November, 1810, when the Ohio basin was a virgin forest. All other high floods have occurred in the early spring, at periods when the "spongy" layers of leaves and moss must have been soaked to saturation by earlier rains and snows. It may be, that changed conditions of the surface, such as the destruction of forests and cultivating and draining the soil may have a sensible influence in increasing the height of summer freshets, which might be termed floods of intermediate height; but in February, when the great floods usually occur, the previously saturated soil and overflowing marshes are merely like other surfaces, the same everywhere, and therefore can have no regulating effect on floods.

Mr. W. Milnor Roberts, in a report on the surveys of the Ohio river,* referring to some views previously expressed by him in a pamphlet printed by the Franklin Institute, and combatting the theories of Chas. Ellet in favor of reservoirs as a means to hold back floods on the Ohio, says: "It was proved from an examination of the records of the floods on the upper part of the Ohio, that some of the highest floods occurred when such reservoirs, had they been in existence, must have been full. Such being the case they could not have aided in restraining those floods, and this would certainly be the case almost every year."

If Mr. Roberts could argue this way in view of the fact that Mr. Ellet's dams across the heads of the Ohio would be, in some instances, 100 feet high, reaching from hill to hill and of enormous storage capacity, he certainly would have placed little reliance on moss, leaves, marshes and forests as restraining reservoirs for spring freshets in the Ohio valley.

Neither Mr. Wex, or any of the authorities I have been able to consult have discussed the question of river floods in connection with meteorological records, the two are, of course, inseparable, and it is therefore the more surprising that distinguished writers will advance an argument on one with no reference to the other, save in general terms.

More has been done by the hand of man on the North American continent in the removal of forests during the last forty years than has probably been accomplished in Europe in a century and a half. Moreover the proportion of mountain land in Europe is considerably greater than it is in the eastern United States. If, therefore, decrease of rainfall, or other meteorological phenomena, are affected by such means, they would be most marked in the region of plains, and particularly in rapidly cleared and settled plains. In the Allegheny valley above this city several billion feet of lumber have been cut in the last forty years, and a vastly larger quantity of timber has been recklessly destroyed by forest fires and by the process of girdling trees in opening up farms.

As Professor Gilbert wisely remarks, "The weather of the globe is a complex whole, each part reacts on every other, and each part of which depends on every other." The rain-gauge records which I have obtained from Sergeant O. D. Stewart, of the signal service office of this city, and

* Ex. Doc. 72, House of Representatives, 41st congress, 3d session.

which I have summarized in the appendix of this paper, show conclusively that during the ten years' time covered by the official reports for thirty-seven specially selected points, in regions east of the Mississippi, where lumbering operations have been greatest, there has been no diminution of the rainfall. In fact, taking the sum of the thirty-seven stations by half decades, the second half exhibits an increase of 1.68 inches of rain. The record of Philadelphia, which I have obtained from another source for a period of sixty-four years, likewise, also shows an increase in the rainfall. So also the combined * Cannonsburg and Pittsburgh record from 1845 to 1884, indicates the same thing.

There are remarkable variations in different parts of the United States in the amount of precipitation of rain and snow, and in the proportion evaporated. Five inches of snow will sometimes completely disappear by evaporation alone in eastern Montana and western Dacotah, and in that region nearly all the rain evaporates, hence perennial streams are found only at long intervals, and even the largest of them, like the Little Missouri and the Mussleshell, dwindle as they leave the elevated regions in which they originate. The great Missouri valley itself, fully twice the area of the Ohio basin, discharges, according to † Humphreys and Abbott, only 3.7 trillion feet as compared with 5 trillion cubic feet per annum by the Ohio into the Mississippi river. Yet, it is the opinion of Professor Cyrus Thomas and many others, that with the advance of settlement in this region, and the cultivation of the soil, that the moisture increases. At all events agricultural operations are being conducted on the great plains of the Columbia river and elsewhere in the west without the aid of irrigation, where a few years ago nothing would grow without artificial aid. The waters of the Great Salt Lake now stand 10 feet higher than they did when Utah was first settled by the Mormons, and this in the face of the fact that several of its tributaries have been heavily taxed in furnishing a supply to irrigating ditches.

The powerful west and northwest winds which traverse our continent in three or four days' time form nearly half of all the winds which reach us, and make up, most probably, 70 per cent. of the total wind mileage which crosses western Pennsylvania. I could not, at the signal office, conveniently obtain the separate mileage of the winds by direction, but I have constructed discs of the year 1883 by quarters, illustrating the comparative duration of the various winds. The wind mileage at Pittsburgh varies from 46 to 55 thousand miles per annum, and I am of the opinion that there is a relationship between northwest winds and rainfall, heretofore, not to my knowledge, duly considered. But not until it is shown that clearing forests affects the winds, and the evaporation from the Pacific ocean, can I believe such an agency exerts an influence on rainfall in this part of the world.

At the last moment before closing this paper my attention was called to an article in "*The Nation*," of February 28, by Mr. H. W. S. Cleaveland. Mr. Cleaveland takes the ground, that in addition to the destruction of forests, our floods are increased by the laying of drain tile in the farms

* Cannonsburg is in Washington county, Pa., 25 miles distant from Pittsburgh. No early records at Pittsburgh are obtainable. T. P. R.

† Hydraulics of the Mississippi.

throughout the west. He believes that within the last few years, in the states of Ohio, Indiana and Illinois, that 100,000 miles of drain pipe have been laid. I would estimate drains laid at intervals of 40 feet, which is over the average distance apart they are usually placed, that 100,000 miles of them would be enough to expedite the drainage of only 750 square miles, an area not much greater than that of Allegheny county. Such an area distributed over three great states like Ohio, Indiana and Illinois, would be lost. There are numerous creeks tributary to the Ohio draining 750 square miles, the addition of another one would be inappreciable either at a high or low stage at Cincinnati.

The following is a stereotyped "fact of history" difficult to controvert:

"The Levant, Algeria, and other regions abutting on the Mediterranean sea were, previous to the Christian era, heavily timbered and densely populated. But since the destruction of the forests they have become almost rainless deserts."

The "historians"(?) neglected to tell us on what food a dense population existed in a forest. In the United States it has been found that by cutting away the timber, room for one hundred or more civilized beings is made on the same ground which gave an Indian hunter a scanty subsistence. This appears to be done also without decreasing the rainfall.

OHIO RIVER FLOODS.

I have appended the records of floods on the Ohio river at Pittsburgh and Cincinnati, so far as I have been able to obtain them. The Cincinnati record is copied from a publication of the chamber of commerce of that city. The Pittsburgh record I have made from various sources, and after consultation with several persons interested in such matters. I have endeavored to reduce them to the Monongahela stone marks opposite Market street, which are recognized to be correct for high water, though not agreeing in the lower stages with other marks.

AT PITTSBURGH.

It will be observed from the records that the flood of 1884 was only four inches higher than that of 1832, 52 years previous. Between the years 1851 and 1867, inclusive (omitting two years of which records are not known to exist), there were eight floods reaching 25 feet or over, five of which were over the 30-foot mark. The mean high water for the sixteen years was 24.7 feet.

During the second period viz: from 1868 to 1884 inclusive, there were but four floods reaching as high as 25 feet, and not until the recent great flood did the river rise to 30 feet or over. The high water mean for the period is 23.6 feet.

The low water mean appears from the records from 1855 to date, when divided into two equal periods to have been the same for both, viz.: 8 inches. The lowest water in the Ohio river at this point known to have been gauged was in 1838,* when Captain John Sanders, U. S. A., in charge

* In 1838 the Ohio at Pittsburgh was, during 32 days, at or below a depth of 1 foot in the channel.

of works of improvement stated it to have been 85,000 cubic fee per minute. Captain F. A. Mahan, U. S. engineer in charge of the Davis island dam,* and member of our society, has, on various occasions, gauged the low water discharge of the Ohio in periods of drouth within the past few years. The lowest he reports was during the year 1879, viz.: 96,000 cubic feet per minute, but Mr. Wm. Martin, Captain Mahan's assistant, and also a fellow member of our society, reports that in 1881 the water was evidently lower, though it was not determined that year what the least discharge was.

There seems to me to be nothing in the river records of Pittsburgh on which to establish any theory, either for increase in the height of floods, or for less low water discharge, due to the destruction of forests or to any other cause.

The records of the Monongahela Slackwater Navigation Company, since 1841, do not indicate a tendency towards a decline year by year of the low water discharge. Quite a number of insignificant brooks are reported dry or lower than formerly in Washington and Greene counties, but their low water discharges must, at their best, have been trifling. The West Fork river, a principal prong of the Monongahela, draining about 900 square miles, is even yet a heavily timbered district, but years ago it was reported of this stream that in seasons of protracted drought its entire discharge "would pass through a hole 2 inches in diameter under 6 inches of pressure." Its valley is flat and its pools undoubtedly suffer much loss from evaporation. But rivers of considerable low water volume are certainly not dependent on tributaries, which like some of those of the Monongahela, literally dry up. The Cheat, the Tygart's Valley and the Youghiogeny—others of its tributaries—which originate in elevated open glade regions, are not likely under any circumstances to evaporate.

It is further to be recollected that a river of large drainage is made up from a multitude of tributaries. Even during continued droughts local showers alternate now on this branch, now on another branch. A given locality may not be visited by one of them for two weeks or more, but the aggregate effect of these passing rain clouds results in keeping up more or less the equable low water discharge of the river into which they pour. The low water discharge being to a great extent maintained, we might say, by a circulating series of minor freshets from numerous tributaries.

During a season of drought on the Allegheny in 1878, when the low water discharge was about 100,000 cubic feet per minute, I was surprised to find that the Clarion and Kis-Kiminitas, ordinarily large tributaries, were almost dry, the Clarion discharging approximately 1,500 cubic feet per minute, and the Kis-Kiminitas 5,000; whereas French creek, which has a smaller basin than the last named, was discharging over 40,000 cubic feet per minute. French creek heads in an elevated plateau of open country. Fort de Boeuf was located in this plain by the French as early as 1750, the name being given from the fact that it was an open, grazing, buffalo country. I cite these instances, not to dispute or prove any theory, but merely to emphasize the importance of considering the topography, and

* Davis island dam across the Ohio river is located four miles below Pittsburgh.

particularly the elevation and mean temperature of regions, before empirically asserting that the equable discharge of streams originating therein may be seriously deranged by human agencies.*

AT CINCINNATI.

The river record of Cincinnati extends only from 1858 to 1884, inclusive. During the first period, viz.: from 1858 to 1870, inclusive, five floods exceeded 55 feet in height, the highest being in 1862, when the water reached to 57 feet 4 inches. Means of the period, 49 feet 11 inches.

During the second period, viz.: 1871 to 1883, inclusive, only three floods exceeded 55 feet (or four with the late flood included). The mean of high waters was 49.9 feet, or, including the late flood, the mean would be 51 feet 3 inches, as compared with 49.11 feet of the first period. Two such phenomenally high floods occurring in two successive years seriously affects the average for a period as short as seventeen years. They stand alone, however, and unless wonderful works by the hand of man in two years can be shown to have changed the face of nature in the Ohio valley, I can see no propriety in introducing them as factors in this discussion. We cannot, it is true, ignore the record they make, but in the light of the 35 years' record preceding, I cannot see that any one would have been safe in predicting their appearance.

The mean annual and low water depths indicate a slight decline in the last period as compared with the first, in this respect, supporting the theory of Mr. Wex; but in the low water record the difference, 2 inches, is so slight that it might be offset by even one year's time of water slightly better than the average. I am not aware of the fact, if it be true, that any gaugings of the low water discharge of the Ohio have been made at Cincinnati.

CONCLUSION.

The great flood of 1884 on the Ohio stands on the record as the highest known. It occurred at the usual period of the year for floods, viz.: the early spring, when the ground is almost invariably soaked with water from previous rain and snows. Even if the entire Ohio valley had been a forest, undrained, and abounding with swamps, the snows and rains which produced this flood would have fallen upon water-soaked leaves and marshes unable to hold more, and therefore not to be accounted restraining reservoirs. A heavy mantle of snow on the ground will, in this latitude, thaw the earth under it and gradually melt down. If additionally to this the snow is exposed to the sunlight and unobstructed winds it will melt away in advance of that in the deep recesses of a forest. Snow will accumulate all winter in such sheltered places, increasing in depth and apt, with the

*In a report to the commission of engineers on water supply at Philadelphia, 1875, James F. Smith states that in 1816 the Schuylkill river flowed, at its lowest stage, 500,000,000 gallons per day. In 1825 it was stated to be 444,000,000 gallons; in 1867, 400,000,000 gallons; in 1874, 275,458,000 gallons per day. Mr. Smith thinks it probable that the early calculations were only approximately correct. Owing to the extensive use of the Schuylkill by numerous manufacturing establishments, Mr. Smith was able to obtain his accurate figures only by many separate calculations. No reference to the increase in the height of floods on the Schuylkill is made by him. T. P. R.

first general thaw, which respects neither field nor forest, to produce freshets in the brooks. I have observed, in the Cumberland valley, of this state, where forests and cleared lands alternately checker the country, that the fields are frequently bare of snow at the time of the annual spring floods of the creeks.

Moreover, if it be said that road-side ditches and other artificial drains expedite the drainage of the country into the river, we must recollect that the velocity of the river current itself will thereby be increased also. There is quite a difference in the velocity of streams, particularly near their head waters, dependent upon the time of this rising to a given high stage. The descending but advancing slope from the crest being steeper when the rise is sudden. The records of this particular flood at several points show that the river was moderately high, when the flood began, hence the wave came slowly. Rains were daily and universal for about two weeks altogether in the upper part of the valley, and continued a week after the danger was over at Pittsburgh, but not too late to augment the flood at Cincinnati, where, unfortunately, the river is scarcely any wider than it is at Pittsburgh, 468 miles further up. The area covered by the preceding great snow storm was immense, and it may never occur again, that the same region will be so snow-covered, and this was followed by a combined thaw and protracted rainy season. There must be a concurrence of these things before the flood of 1884 in the Ohio valley will be duplicated.

The popular opinion no doubt will long be that the destruction of forests increases the height of floods, but I am persuaded it is not a belief founded on established facts.

TABLE SHOWING HIGHEST, LOWEST AND MEAN OF WATER AT PITTSBURGH, AND AVERAGES FOR TWO PERIODS.

DATE.	Year.	Highest.	Mean.	Lowest.	REMARKS.
Nov. 10....	1810	32.	....	....	
Jan........	1813	29.	....	....	
Feb........	1816	33.	....	....	
Feb. 10....	1832	34.	....	....	
Feb. 1....	1840	26.9	....	....	
Feb. 1....	1847	26.	....	....	
Sept. 20....	1851	30.9	....	....	First Period, 1847 to 1863. Average for the period is 24 ft. 7 in. High water.
April 19....	1852	31.9	....	....	
	1853	31.9	....	....	
	1855	18.0	....	2.10	
	1856	19.6	....	0.3	
	1857	21.4	....	0.0	
	1858	26.0	....	0.5	
	1859	22.0	....	1.1	
April 12....	1860	29.7	....	2.8	First Period, 1855 to 1870. Average for the period is 8 in. Low water.
Sept. 29....	1861	30.9½	....	1.10	
Jan. 20....	1862	28.7	....	0 3	
Jan. 16....	1863	16.0	....	0.1	
Dec. 17....	1864	18.6	....	1.0	
March 8...	1865	31.4	....	1.4	
Feb. 25....	1866	15.4	....	0.4	
March 13..	1867	22.6	....	0.0	
April 15....	1868	20.6	5.1	0.0	Second Period, 1868 to 1884. Average for the period is 23 ft. 6 in. High water.
March 31..	1869	19.6	6.2	0.7	
Jan. 19....	1870	18.0	....	1.3	
Jan. 15....	1871	19.0	....	1.2	
April 11....	1872	20.6	....	1.6	
Dec. 14....	1873	25.6	....	1.6	
Jan. 8....	1874	22.4	....	1.1	
Aug. 3....	1875	25.0	6.15	0.4	
Sept. 19....	1876	26.0	5.88	0 2	Second Period, 1870 to 1884. Average for the period is 8 in. Low water.
Jan. 17....	1877	25.0	4.95	0.7	
Dec. 11....	1878	24.6	5.20	0.0	
March 19..	1879	20.0	4.30	0.1	
Feb. 14....	1880	22.0	4.70	0.3	
Jan. 10....	1881	28.0	4.96	0.6	
Jan. 28....	1882	21.9	5.11.6	0.6	
Feb. 8....	1883	27.6	5.11.3	0.1	
Feb. 6....	1884	34.4⅛	....	....	

STATEMENT SHOWING THE HIGHEST, LOWEST, AND AVERAGE STAGES OF THE OHIO RIVER, AT CINCINNATI, EACH CALENDER YEAR FROM 1858 TO 1883, INCLUDING FLOODS OF 1832 AND 1847.

CALENLER YEAR.	HIGHEST STAGE.		LOWEST STAGE.		AVERAGE.
	Date.	Ft. In.	Date.	Ft. In.	Ft. In.
1832..........	February 18......	64.3	....	....	....
1847..........	December 17.....	63.7	....	....	....
1858..........	June 16...........	43.10	October 3...	2.5	12.10
1859..........	February 22......	55.5	September 19....	3.3	17.7
1860..........	April 16...........	49.2	October 3........	5.4	16.
1861..........	April 19...........	49.5	July 3............	5.1	19.1
1862..........	January 24.......	57.4	October 31.......	2.4	17.5
1863..........	March 12.........	42.9	October 6........	2.6	15.
1864..........	December 23.....	45.1	August 6.........	3.1	16.8
1865..........	March 7..........	56.3	October 19........	5.8	21.10
1866..........	September 26.....	42.6	August 17........	4.9	19.2
1867..........	March 14.........	55.8	October 19.......	3.0	17.
1868..........	March 30.........	48.3	July 21..........	5.1	18.8
1869..........	April 2............	48.9	August 21........	5.4	19.8
1870..........	January 19.......	55.3	October 4........	3.10	17.10
1871..........	May 13............	40.6	October 14........	2.8	11.10
1872..........	April 13...........	41.9	October 14........	3.	11.8
1873..........	December 18.....	44.5	October 12........	3.8	18.5
1874..........	January 11.......	47.11	September 29....	2.4	15.8
1875..........	August 6.........	55.4	September 19....	4.3	18.9
1876..........	January 29........	51.9	September 4....	6.2	18.2
1877..........	January 20.......	53.9	October 9........	3.3	15.
1878..........	December 15.....	41.4	October 24........	4.4	16.9
1879..........	December 27.....	42.9	October 23........	2.6	14.6
1880..........	February 17......	53.2	October 28........	3.9	17.
1881.........	February 16......	50.7	September 18.....	1.11	16.11
1882..........	February 21......	58.7	November 1......	6.1	22.1½
1883..........	February 15......	66.4	September 21.....	3.7	19.5½

TABLE SHOWING WAVE OF THE FLOOD OF 1884 AT DIFFERENT STATIONS ALONG THE ALLEGHENY AND OHIO RIVERS. RECORDS TAKEN ON THE SAME DATES.

	January 30.	January 31.	February 1.	February 2.	February 3.	February 4.	February 5.	February 6.	February 7.	February 8.	February 9.	February 10.	February 11.	February 12.	February 13.	February 14.
Oil City..........	2.4	5.0	5.6	5.4	......	6.0	13.0	13.3	12.0	10.2	9.0	......	8.	8.8	8.8	13.1
Freeport..........	5.8	8.0	12.2	11.6	11.6	9.3	14.2	30.0	28.6	22.2	......	18.6	16.10	15.8	15.10	19.8
Pittsburgh	3.4	8.5	20.5	17.4	12.10	11.1	15.10	34.4⅓	31.8	26.3	21.3	18.5	16.11	18.4	18.0	17.10
Wheeling........	8.10	......	20.0	......	......	22.0	......	......	......	......	......	......	30.	.....	29.6	29.0
Marietta.........	......	21.6	21.6	25.6	28.4	25.3	29.0	37.8	46.3	49.6	52.0	48.3	46.1	38.0	35.0	34.0
Point Pleasant ..	......	......	......	......	......	......	......	......	......	......	......	......	......	......	......	......
Portsmouth	......	......	......	.....	......	......	......	......	......	......	......	......	......	......	......	......
Cincinnati	18.1	31.3	38.8	46.2	49.5	49.11	52.8	59.5	61.8	62.7	63.9	64.9	66.5	68.5	69.11	71.1
Louisville	8.0	11.0	13.7	16.10	21.8	24.0	27.8	34.1	38.5	39.8	40.3	40.4	41.1	42.4	44.0	45.7
Evansville.......	......	15.5	18.1	23.6	29.	32.7	35.5	37.9	40.6	42.8	44.1	44.9	45.3	45.6	46.	46.3½
Cairo	24.11	25.8	25.8	26.6	29.5	32.5	34.7	37.10	40.10	42.8	44.	45.1	46.	46.10	47.7	48.4

	February 15.	February 16.	February 17.	February 18.	February 19.	February 20.	February 21.	February 22.	February 23.	February 24.	February 25.	February 26.	February 27.	February 28.	February 29.
Oil City	12.6	10.0	9.0	8.5	6.6	6.0	6.8	5.7	5.7	5.7	5.7	5.5	4.11	4.8	.....
Freeport............	17.4	15.0	12.6	13.0	11.9	11.4	12.4	11.6	9.10	8.10	7.11	7.8	7.3	7.0	......
Pittsburgh	20.10	17.11	14.3	15.4	13.3	12.6	12.6	11.5	10.1	9.1	8.1	7.6	7.2	6.10	......
Wheeling	31.9	28.7	25.5	22.3	19.0	20.6	19.0	19.3	17.5	15.7	13.8	12.0	11.4	......	......
Marietta......... ...	33.6	31.6	29.0	27.0	24.2	23.0	21.0	20.0	19.0	18.0	15.6	14.1	13.2	11.10	9.6
Point Pleasant	......	......	......	......	......	......	......	......	......	......	......	......	......	......	......
Portsmouth	......	......	......	......	......	......	......	......	......	......	......	......	......	......	......
Cincinnati..........	70.1	68.4	66.0	63.5	60.3	58.10	55.5	52.0	48.5	45.2	40.10	36.10	32.11	29.8	......
Louisville	46.6	46.6	45.8	44.5	42.6	41.3	39.1	36.1	32.11	29.3	24.8	21.8	17.6	......	......
Evansville..........	46.10	47.3	47.6½	47.10¾	48.	48.¼	47.11¾	47.8	47.3¼	46.9	46.	45.3	44.	42.6	41.1
Cairo	49.1	49.10	50.5	50.10	51.3	51.7	51.9	51.10	51.10	51.10	51.8	51.6	51 2	50.	......

RAIN GAUGE RECORDS—CANNONSBURGH AND PITTSBURGH.

YEAR.	INCHES.	REMARKS.
1845	27.54	
1846	40.81	
1847	39.61	Cannonsburg is 20 miles southwest from Pittsburgh. The Pittsburgh record begins with 1871.
1848	31.59	
1849	32.72	
1850	31.0	
1851	23.74	
1852	39.48	There are no records for either place in 1870.
1853	25.48	
1854	20.86	
1855	41.91	
1856	25.07	
1857	43.53	
1858	31.59	Average rainfall from 1845 to 1863, inclusive, 35.01 inches.
1859	39.23	
1860	39.43	
1861	36.46	
1862	31.61	
1863	33.58	
1864	39.95	Average rainfall, 1864 to 1883, inclusive, 37.68 inches.
1865	43.90	
1866	36.51	
1867	29.77	
1868	30.61	
1869	36.60	
1870	....	Increase of the second period over the first period is 2.67 inches.
1871	24.35	
1872	30.78	
1873	41.57	
1874	39.34	
1875	34.06	
1876	37.01	
1877	34.72	
1878	38.76	
1879	37.02	
1880	31.97	
1881	37.30	
1882	38.61	
1883	43.17	
1884	....	

TABLE OF RAINFALL IN PHILADELPHIA FROM 1810 TO 1875.

(Report of the Commission of Engineers on the Water Supply of Philadelphia.)

Year.	Rainfall.	Average.	Year.	Rainfall.	Average.	Year.	Rainfall.	Average.	REMARKS.
	Inches.	Inches.		Inches.	Inches.		Inches.	Inches.	
1810...	32.66	First Period.	1840....	47.40	Fourth Period.	1870....	44.11	Sev'nth Period.	
1811....	34.97		1841....	55.50		1871....	47.32		
1812....	39.30		1842....	48.54		1872....	51.12		
1813....	35.63		1843....	46.91		1873....	58.29		
1814....	43.14		1844....	40.17		1874....	40.91	48.35	
1815....	34.67		1845....	40.02		....	....	....	
1816....	27.95		1846....	44.39		....	....	....	
1817....	36.01		1847....	45.09		....	....	....	
1818....	30.18		1848....	35.00		....	....	....	
1819....	23.35	32.79	1849....	42.09	44.57	....	....	....	
1820....	39.61	Second Period.	1850....	54.54	Fifth Period.	....	....	....	
1821....	32.18		1851....	35.50		....	....	....	
1822....	29.86		1852....	45.75		....	....	....	
1823....	41.85		1853....	40.66		....	....	....	
1824...	38.74		1854....	40.18		....	....	....	
1825....	29.57		1855....	44.09		....	....	....	
1826....	36.15		1856....	33.93		....	....	....	
1827....	38.50		1857....	48.29		....	....	....	
1828....	37.97		1858....	39.85		....	....	....	
1829....	41.85	36.63	1859....	58.12	44.09	....	....	....	
1830....	45.07	Third Period.	1860 ...	44.09	Sixth Period.	....	....	....	
1831....	44.94		1861....	46.44		....	....	....	
1832....	39.87		1862....	45.01		....	....	....	
1833....	48.55		1863....	49.19		....	....	....	
1834....	34.24		1864....	46.00		....	....	....	
1835....	39.30		1865....	56.26		....	....	....	
1836....	42.66		1866....	45.26		....	....	....	
1837....	39.04		1867....	61.19		....	....	....	
1838....	45.24		1868 ...	51.40		....	....	....	
1839....	43.74		1869....	48.86	49.37	....	....	....	

AVERAGE RAINFALL OF THIRTY-SEVEN POINTS IN THE UNITED STATES.

Divided into Two Periods—First, from 1871 to 1875 Inclusive; Second, from 1876 to 1880 Inclusive.

PLACE.	AVERAGE.		Increase.	Decrease.
	First Period.	Second Period.		
	Inches.	Inches.	Inches.	Inches.
Breckenridge, Minn	25.20	25.59	.39	....
Cairo, Ill	44.47	46.36	1.89	....
Chicago, Ill	33.06	35.45	2.39	....
Cincinnati, O	39.06	47.03	7.97	....
Cleveland, O	35.19	41.61	6.42	....
Detroit, Mich	28.95	40.77	11.82	....
Duluth, Minn	32.68	35.61	2.93	....
Escanaba, Mich	33.58	34.14	.56	....
Grand Haven, Mich	35.64	37.47	1.83	....
Indianapolis, Ind	46.36	45.82	....	.54
Knoxville, Tenn	56.81	48.96	....	7.85
LaCrosse, Wis	33.32	34.31	.99	....
Marquette, Mich	28.06	33.87	5.81	....
Memphis, Tenn	50.31	58.45	8.14	....
Milwaukee, Wis	30.72	37.93	7.21	....
Morgantown, W. Va	47.84	45.45	....	2.39
Nashville, Tenn	50.90	54.00	3.10	....
Pittsburgh, Pa	38.48	35.89	....	2.50
St. Louis, Mo	35.78	38.01	2.23	....
St. Paul, Minn	32.88	29.48	....	3.40
Toledo, O	29.67	33.67	4.00	....
Vicksburg, Miss	58.97	60.42	1.45	....
Buffalo, N. Y	34.03	40.74	6.71	....
Dubuque, Ia	38.85	37.49	....	1.36
Erie, Pa	41.22	42.09	.87	....
Fort Gibson, Ind. Ter	39.48	36.78	....	2.70
Galveston, Texas	52.12	51.32	....	.80
Indianola, Texas	38.94	38.59	....	.35
Keokuk, Ia	38.65	37.81	....	.84
Leavenworth, Kan	35.96	42.02	6.06	....
Lynchburg, Va	42.35	40.88	....	1.47
Montgomery, Ala	58.45	52.08	....	6.37
Omaha, Neb	32.19	33.86	1.67	....
Oswego, N. Y	31.28	40.40	9.12	....
Port Huron, Mich	32.97	35.37	2.40	....
Rochester, N. Y	37.61	39.15	1.54	....
Yankton, Dakota	29.97	25.38	....	4.59
	1431.90	1494.15	97.50	35.25

NOTE.—The general average for the 37 stations above mentioned is 1.68 of an increase.

TABLE SHOWING HIGHEST STAGE OF WATER ON FIVE GERMAN RIVERS, FROM 1728 TO 1871.

YEAR.	Elbe at Magdeburg.		Rhine at Emmerich.		Oder at Kustrin.		Vistula at Kurzebrack.		Danube at Vienna.	
	Ft.	In.	Ft.	In.	Ft.	In.	Ft.	In.	Ft.	In.
1728	14	3.97								
1729	16	2.62								
1730	15	7.93								
1731	16	8.80								
1732	14	11.18								
1733	13	3.61								
1734	15	5.69								
1735	16	5.20								
1736	16	9.32								
1737	16	9.32								
1738	16	5.20								
1739	15	10.5								
1740	16	11.63								
1741	17	3.75								
1742	16	1.59								
1743	13	11.85								
1744	17	2.98								
1745	16	11.89								
1746	15	8.78								
1747	17	0.92								
1748	17	1.95								
1749	16	0.05								
1750	16	8.29								
1751	16	10.09								
1752	16	4.42								
1753	16	10.86								
1754	17	2.98								
1755	17	5.04								
1756	16	7.26								
1757	15	1.24								
1758	16	2.62								
1759	11	4.70								
1760	16	2.62								
1761	17	3.75								
1762	16	7.26								
1763	13	0.52								
1764	16	5.20								
1765	14	8.09								
1766	13	5.67								
1767	15	4.33								
1768	16	8.28								
1769	17	1.95								
1770	17	1.95	22	10.94						
1771	17	6.58	19	6.78						
1772	16	7.26	20	9.20						
1773	13	3.61	19	6.78						
1774	16	2.62	20	9.20						
1775	17	7.61	20	10.23						
1776	16	5.36	20	8.17						
1777	16	4.42	20	4.05						
1778	14	0.88	21	6.47	9	3.21				
1779	15	7.93	21	6.47	9	11.97				
1780	17	3.66	19	4.72	13	7.73				
1781	17	6.58	23	2.03	9	2.70				
1782	14	9.46	19	3.18	8	4.92				
1783	17	6.58	22	3.22	11	1.88				
1784	17	5.72	24	7.02	11	5.99				
1785	18	4.11	18	1.28	15	6.38				
1786	15	11.53	18	10.54	11	2.38				
1787	12	5.05	19	10.59	9	11.97				
1788	12	5.99	17	4.01	9	11.45				
1789	17	1.95	21	1.68	10	11.29				
1790	13	7.22	19	5.75	7	11.77				
1791	10	11.81	21	4.47	5	7.96				
1792	12	2.22	23	0.49	7	8.16				
1793	12	10.72	19	1.63	7	6.10				
1794	16	4.68	18	3.34	10	9.75				
1795	13	3.61	23	6.15	6	11.41				
1796	13	3.61	19	2.66	5	11.05				
1797	13	4.43	16	5.71	6	4.72				

TABLE SHOWING HIGHEST STAGE OF WATER, ETC.—*Continued.*

YEAR.	Elbe at Magdeburg.		Rhine at Emmerich.		Oder at Kustrin.		Vistula at Kurzebrack.		Danube at Vienna.	
	Ft.	In.	Ft.	In.	Ft.	In.	Ft.	In.	Ft.	In.
1798	14	1.91	19	1.63	10	5.11				
1799	17	7.61	25	0.69	10	5.63				
1800	12	8.92	19	1.63	8	3.88				
1801	13	1.55	22	4.76	8	6.97				
1802	15	8.78	21	2.35	9	0.12				
1803	15	8.78	23	7.18	7	6.62				
1804	16	10.86	21	8.53	10	2.02				
1805	17	9.50	24	5.48	11	4.96				
1806	15	5.00	21	11.67	8	8.26				
1807	17	3.75	22	1.67	10	6.14				
1808	17	7.61	19	5.75	9	1.15				
1809	17	10.19	24	11.66	9	0.12	17	4.01		
1810	16	11.89	20	1.99	9	11.45	15	11.53		
1811	13	5.32	20	10.23	7	4.56	12	9.95		
1812	16	4.68	20	9.20	5	11.05	13	4.13		
1813	15	4.06	16	10.86	11	5.99	22	9.91		
1814	18	1.28	24	7.50	13	4.13	17	10.70		
1815	15	3.30	18	11.57	6	1.11	15	9.47		
1816	13	5.67	20	5.08	9	9.91	21	2.35		
1817	14	10.15	21	10.59	9	1.67	16	10.86		
1818	10	11.81	19	5.75	6	11.41	20	1.99		
1819	14	1.91	22	8.88	7	0.95	14	5.26		
1820	17	10.19	22	11.97	9	3.21	15	9.47		
1821	15	10.50	19	3.69	7	9.19	18	7.46		
1822	11	8.05	15	11.53	7	5.59	20	4.05		
1823	11	7.02	17	8.13	7	6.62	17	6.07		
1824	17	0.92	23	6.15	6	0.08	14	3.97		
1825	11	0.84	20	1.99	6	2.66	13	0.01		
1826	12	9.43	14	4.99	7	11.77	13	4.64	7	10.41
1827	17	10.19	21	8.53	9	1.15	20	6.11	8	6.71
1828	16	2.62	18	7.45	8	9.03	20	7.14	8	2.56
1829	13	8.76	16	11.89	9	3.39	24	0.84	9	1.97
1830	18	0.25	22	10.94	12	11.49	20	10.49	7	0.36
1831	16	4.68	22	3.73	10	0.48	16	9.83	8	1.52
1832	12	4.28	19	3.69	8	2.86	6	9.35	7	7.30
1833	16	3.65	23	1.00	10	1.51	15	7.42	8	7 75
1834	16	10.86	23	1.00	10	7.69	17	9.16	8	3.60
1835	8	4.92	15	2.26	4	2.46	8	4.92	6	7.89
1836	13	7.73					13	6.18	7	0.36
1837	16	6.74					19	4.21	7	11.45
1838	17	4.00					19	2.66	6	5.81
1839	16	0.56					20	3.02	7	11.45
1840	15	8.44					16	6.74	9	3.01
1841	17	5.04					21	2.86	7	3.15
1842	13	11 85					8	3.88	5	5.36
1843	14	8.09					16	1.59	7	10.41
1844	18	0.25					22	1.67	6	2.70
1845	19	1.63					22	2.70	9	1.97
1846	18	2.31					20	10.23	8	6.71
1847	17	10.19					10	9.75	8	0.49
1848	16	10.86					14	0.88	8	2.56
1849	12	6.34					19	4.72	8	9.82
1850	18	4.37					18	9.52	6	6.85
1851	16	2.62					16	9 83	8	2.56
1852	17	4.01					12	9.43	7	2.11
1853	16	10.86					17	9.16	9	5.09
1854	16	7.77					23	2.54	5	8.48
1855	18	0.25					28	3.81	7	11.45
1856	16	3.65					13	11.85	7	10.41
1857	8	3.88					13	5.67	4	9.06
1858	15	1.24					13	6.70	7	10.41
1859	12	6.34					12	4.28	6	8.93
1860	17	8.13					19	8.87	6	4.77
1861	13	5.67					19	0.60	8	6.71
1862	18	10.54					13	6.70	12	2.29
1863	10	8.72					6	3.17	5	3.29
1864	10	11.81					10	10.78	6	6.85
1865	18	0.25					18	3.34	6	0.62
1866	9	11.45					9	5.27	4	8.02
1867	16	3.65					20	7.14	8	2.56
1868	13	5.67					20	1.99	7	2.26
1869	12	10.46					11	5.99	5	9.51
1870							21	1.31	6	8.93
1871							21	8.53	10	11.76

DISCUSSION

OF MR. ROBERTS' PAPER ON THE SUBJECT OF THE DESTRUCTION OF FORESTS, CAUSING INCREASE IN THE HEIGHT AND FREQUENCY OF FLOODS, MARCH 18, 1884.

MR. BUCHANAN said: There is one thing about that Cincinnati diagram I would like to ask Mr. Roberts. They had high water in 1883. I only see one unusually high stage. Are the two shown together?

MR. ROBERTS:—I omitted that of 1884. I did not know how much higher it was going to get this year, but I included that of 1884 when I stated that the mean of the second period would be one foot higher. Without the flood of 1884 included, the mean of high waters at Cincinnati is forty-nine feet nine inches, two inches lower than the first period, but when we add the 1884 flood then it brings the second up one foot higher than the first period. I did not include it in the diagram.

MR. BROWNE said: I do not see that these records which simply give the elevations of water at given times prove very much. I do not think that they can prove sufficient for us to base any theory upon, for this reason. The area of the channel has been considerably diminished since 1832. The only difference in the stages of water recorded was four or five inches. Now we would infer from that that the flood of 1884 was greater than the flood of 1832, which every one knows was not the case. Take the river—the inroads the railroads have made and the diminution which the channel has undergone in that time, and it becomes clear that the volume of water at Pittsburgh was fully one-third more in 1832 than 1884. The only way by which reliable data could be obtained for the purpose of comparing floods in the Allegheny, Monongahela, and Ohio rivers would be to obtain the average discharge of different points during different times, and in that way some logical inference might be drawn, but these profiles cannot give anything even approximately.

MR. ROBERTS said: The diagram, Mr. President, I have made from Mr. Wex's tables and records of the German rivers. They do not represent the discharges of streams except in a few instances. They have very seldom been gauged accurately. I refer in my paper to the gauge of 1838, which was the lowest on record on the Ohio river. The theory in regard to increased height of floods caused by the destruction of forests is that not only the floods are greater, but the mean of the water is less, and I pointed to the fact that in 1838, Major Sanders, in charge of the Ohio river improvement, had found the discharge to be 85,000 cubic feet per minute. Since then the least known measured discharge was by Captain F. A. Mahan, U. S. Engineers, and member of our society, was 96,000 feet per minute, at Davis' island dam, four miles below this city. Unfortunately we have no other records of official gaugings of the Ohio.

Mr. Browne:—Another point—from the examination of the profiles it would simply go to prove the theory that the removal of forest timbers had greatly increased the height of floods. After the ground has been cultivated for some years it will more readily absorb the moisture than if the original surface had remained intact. It seems apparent, however, from a cursory examination of the record of the last thirty years that the profiles show more water than in the previous thirty.

Mr. Roberts:—What I have sought to present is the data of high water records. The point of the discussion, the great point the people want to have information upon is the high water mark, a very palpable mark that involves property.

Mr. Browne:—There is but one thing evident. If it is continued to make inroads on the river as has been the case for the last few years, in the next fifty years such floods will reach to the moon.

Mr. Buchanan:—It is usually supposed that these unprecedented floods are caused by such things as the destruction of forests. Is it not rather the freshets that are attributed to that cause? Now this year we had seven snow-falls, making the heaviest snow-fall and the deepest snow on record, I believe, in this end of the Ohio valley. It melted partly and froze. Then we had five rain-falls in very close succession. Now, if the whole valley had been covered with forests we would still have had the floods, because in that case the timber would not have made any difference. The idea has been that the destruction of the forests allowed heavier rain-falls, that the clearing of the ground for tillage purposes in cases of heavy rain-falls, would produce sudden freshets. I understand it was generally supposed that floods of this character were affected by destruction of forests. Mr. Roberts spoke as though the opinion of the advocates of forestry was that the despoliation of timber lands has a great influence on floods of phenomenal magnitude. I do not understand it to be such.

Mr. G. C. Henning:—I believe it was stated in the paper that the level of Salt Lake was rising, and this was ascribed to the cultivation of the soil in the vicinity. Reference to the geological survey of 1859 or 1860 shows that Salt Lake is continually undergoing undulations, that is, it rises and falls in periods, and that at present it is simply on its upward movement. From the salt levels on its shores it is seen that the water has risen as much as twelve feet higher than it is at present, and it is supposed when the water reaches that height it will again recede. It is the same in other salt lakes, that is, lakes that have no visible outlet. In regard to the other point, that there was more water now than formerly, also ascribed to cultivation, I would like to add these facts: I believe when the engineers of the Union Pacific and Central Pacific first went into the Rocky Mountains, i. e., mountain plateaus of Utah and Colorado, there was no rain whatever and no wells. After the rails had been down for a few years rain fell, occasionally with lightning, and the more rails were put down the more was the annual rain-fall. Formerly there was no rain at all anywhere on the plateaus. The rain-fall has been increasing, and it seems to bear out the argument that the laying of rails of metal across the plain has had an appreciable effect on rain-fall. I have never heard it stated that the cultivation of the soil in these regions had that influence on the level of Salt Lake or on the annual rain-fall.

Mr. Browne:—Then this salt Lake is liable to recede again. I was greatly encouraged by Mr. Roberts' reference to it, when I heard it had raised ten feet. I was about to ask how long it would take to envelope Salt Lake City. Was sorry to hear it undulates.

Mr. Roberts:—The foundation for my assertions about Salt Lake is from a very interesting, accurate, and valuable report of Professor G. K. Gilbert, in "Lands of the Arid Regions," in Powell's Report. The gentleman further spoke of the undulations in the lake. That is true. It undulates about twenty inches a year, but there is a progressive tendency to rise. It occasionally goes back lower than it was two or three years before, but it now stands ten feet higher than it did in 1856. I have been on the shores of Salt Lake myself and I have seen fence posts sticking out, and I was told by Mormon farmers that they formerly cultivated land now submerged. I have thus, in a manner, witnessed that the waters are higher than they were twenty-five years ago. Professor Gilbert gives a diagram of all the years of which there are records, and hereafter more accurate calculations will be made. But the most wonderful thing of all is the fact that some of the tributaries have been robbed entirely of their discharge for the purpose of irrigating the land, and that still the waters of Salt Lake should rise. I did not discuss it at length, but it occurs to me now that the question what causes increase of moisture in the west might be solved in the planting of corn, which has something of the effect of the forest in retarding evaporation in the heats of summer. After the grain is up a few inches they seldom have to irrigate the land again. This cultivation keeps the moisture in the ground and distributes it something like a forest might do, and I would therefore attribute the increase of moisture more to that account than I would to the railroad theory. I have heard of that and know it to be a fact that wherever they advance railroads they abandon irrigation. With the railroad it seems to come up like the red clover driving away the white, but we must not forget that it is the railroad that takes the settlers and agriculturists to the west.

Mr. Mann:—If it is a fact that railroads increase the rain-fall, would not that account for the recent floods here, considering the number of railroads in and around Pittsburgh, and the number of rails laid. That seems to me a question that naturally follows.

Mr. Lowry:—How would railroads account for the great flood of 1832, when there were no railroads within 360 miles of Pittsburgh?

Mr. Strobel:—The particular point of Mr. Roberts' paper seems to me to be this: That the cutting down of forests, which has been going on so extensively for a century or so, has not resulted in higher floods than were formerly known; his statistics show this. For a number of years our newspapers have been calling attention to the importance of maintaining our forests, a society has been formed to promote forest culture, and Congress has taken measures in this direction. The chief argument has been that forests prevent the occurrence of very high and very low stages of water in our rivers, and that unless steps are taken to protect our trees from the rapid destruction now going on, the country will periodically be affected with devastating floods and scarcely less disastrous drouths. Now it seems there is nothing to prove that such will be the case, and much to show that such results need not be expected. Mr.

Roberts' interesting paper is a valuable contribution in this direction. Mr. Wex is, I believe, considered one of the strongest exponents of the forest theory, and I think Mr. Roberts has shown that he does not prove what he asserts. Wex's book has been severely criticized in other quarters, and his own statistics used to refute the theories he advances. This has been done by a German writer named Herrich, in the proceedings of the Hungarian Society of Architects and Engineers, a resumé of which can be found in the proceedings of the Society of Architects and Engineers, of Hanover, for 1876, from which I have extracted the following notes: Herrich shows that this subject has received much attention in France particularly; that Wex's theories are old, but have not been generally shared by the most competent students of the subject. As regards the theory that the destruction of forests has increased the high waters of rivers, the flood records of the Seine are cited, which are authentic and very complete. They show a gradual lowering of the high water mark much more pronounced than the cases cited by Mr. Roberts, as will be seen from the following:

YEAR.	1615.	1649.	1651.	1751.	1799.	1807.	1850.
	Meters.	Meters.	Meters.	Meters.	Meters.	Meters.	Meters.
High Water....................	9.14	7.65	7.85	6.70	6.97	6.66	6.07

This is a gradual lowering of the highest flood marks of this river of *three meters* in 235 years. Mr. Roberts in his paper treats of only one of Mr. Wex's theories, viz: the effect of forests upon the high and low water stage of rivers and streams. Wex does not rest his case here. He also claims that forests induce atmospheric precipitation so as to increase the amount of rain-fall, and further, that they bring about a more equable temperature by moderating the extreme cold of the winter. Herrich shows that proof is lacking for these theories also, and that rather the reverse appears to have taken place. As regards the rainfall, Monesthier Savignat, a defender of forest culture, and Volles, an opponent, both concur that an increase of atmospheric precipitation has taken place in France, the average for the last century being 480 to 550 mm, and for the present century, 560 to 620 mm, and Flanguerges states the rain-fall at Verviers for the last century to have been 842 to 899 mm. and for the present 926 to 1,012 mm. We have, therefore, here also the direct opposite of the result claimed by Mr. Wex. I would like to ask Mr. Roberts whether he has made any investigation of the subject of the influence of forests on temperature, one of the arguments advanced in their favor by Wex.

Mr. Roberts:—I was very much gratified to hear that corroborative evidence of the side that I have taken in this discussion. It is a valuable statement, and I hope that our reporter will get it all down carefully. In regard to the question about the temperature above forests, I have thought about that, but for want of data I would not like to advance a theory. I think, however, that the temperature 100 feet above the trees

would be about the same as at the same elevation over any neighboring open field. Otherwise we would have to grant at least a part of the theory that the vapors of forests bring down the rain-fall, which the records do not show.

Mr. Prentice:—I have listened with pleasure to the discussion here this evening. I have been reading recently an account from Egypt where over 20,000,000 trees had been planted there, which had had the effect of increasing the rain-fall from six to forty inches, and I think that this shows that the planting of forests or the protection of forests increases the rain-fall, at least in that country. There is now pending before the Legislature of New York a bill to protect the forests in the vicinity of the Mohawk, Black, and Hudson rivers, on account of the decrease of the flow of water in those streams. I cannot recollect the paper in which I read the account concerning the rain-fall in Egypt, but it struck my mind very forcibly at the time, and if it will be of any benefit to our society I can look it up. It showed a heavy increase of rain-fall.

Mr. Strobel:—I may say in regard to the effect of forests on temperature, that Herrich furnishes interesting data on this subject also, from which it appears that the extremes of temperature in Europe have not been greater during the last 150 years than they were formerly; on the contrary they were less. The following notes will, I think, prove interesting reading, irrespective of their bearing on the subject under discussion: In 860 it is reported that the Adriatic was frozen over at Venice, and that the temperature was 20° below zero (Centigrade). In 1234 the Rhone and Po were frozen over and wagons crossed the lagoons at Venice. In 1408 the Danube was frozen over, also the sea between Denmark and Norway. In 1468 troops in Welland are said to have been given their rum in chunks, instead of in liquid form. In 1493 the harbors at Genoa and Marseilles were frozen over, and in 1726 Danes crossed in sleighs from Copenhagen to Sweden. Nothing like these low degrees of temperature has occurred in Europe in the last 150 years, so that in this respect also forests appear to have been of no avail. All these circumstances simply show that the destruction of forests in Europe and this country cannot be proved to have unfavorably affected the floods of the rivers, the annual rain-fall, or the climate. It must be borne in mind, however, that these conditions might be much altered if the cutting down of our trees implied the conversion of forest lands into barren wastes. In this country and in England the lands which have become denuded of trees now support other forms of vegetation which have proven more profitable to the agriculturalist. It is this change with which we are dealing, and instead of deploring the results there appears to be rather cause for congratulation.

Mr. Davison:—The profiles shown are interesting only so far as they show the highest water marks in those particular years. I think that if records could be gotten at and profiles shown of the gauges on a number of days preceding and after those high water marks and the height of those profiles above certain assumed lines were compared that there might be some little light thrown upon some of these theories of high water marks and the amount of the discharge of our rivers. For instance: If we had had the gauges on a number of days preceding and after the high water mark of 1832, and the gauges for the same number of days preceding and after

that of 1884, we might have found that the average of either of those sets of gauges above, say a twenty-five or even a twenty foot gauge would have been much higher than the average of the other set, which, of course would not show us accurately the relations of the amount of water carried down by our rivers in those two different periods, but it would have given us the comparative amounts during those days. The discharge for the same gauge would not be exactly the same even if the gauge read the same for two different days, but it would give us a very good idea of the comparative discharge of those days. I do not know whether Mr. Roberts or any other gentleman here could give us that information as to the number of days that the gauge was high in 1832, and the number of days it was high in 1884, but if we had this information we could determine much better the comparative quantities at those high water marks and could then tell whether it is actually a fact there was more water carried down in one of those freshets than in the other. I think such a profile would show even more than one where the lines are drawn from one high stage to another.

Mr. Roberts:—I would like to point out particularly, in answer to that gentleman, a little thing on this diagram. The water was swifter here at Pittsburgh on the 18th of February when the river was considerably lower than during the flood day. I account for the fact in this way, namely, that the water was higher at Freeport than it was at Pittsburgh, and thus the slope and the actual fall was greater, hence resulted increase of velocity. I did not measure its velocity that day, though my attention was called to it by others. On the following day I measured it and found it was running at about $8\frac{2}{10}$ miles per hour between certain bridges here. On the preceding day, from the information of others who took a little interest in the matter, I have no doubt it was much greater. At similar stages—as regards depth—vastly different amounts of water may be passing down the river, owing to the difference in velocity. If the water is low here and high above you will have a greater slope, and therefore, mere records of depths will be no criterion of the volume of water passing. It would be interesting and valuable for us to know the difference of forest acreage on the headwaters of the Ohio between 1832 and the present time, then in addition to know exactly the rain-fall, its depth, and the areas concerned in making these two great floods. But as this is manifestly impossible for us ever to know, we must content ourselves with the mere records of their muddy lines on the sides of our houses.

Mr. Ackenheil:—Mr. President, I think the destruction of forests has no influence whatever on extreme high or low waters, but has a great influence in destroying springs and small water-courses. Extreme high and low waters are caused by meteorological changes and atmospheric disturbances, which are beyond men's control. We can do nothing about these heavy rain-falls. They had them thousands and thousands of years ago, and we will always have them.

Mr. Lowry:—It is a fact that we seldom have high water in both the Allegheny and Monongahela rivers at the same time. There comes a rise in both rivers, but one goes down before the other comes up. We may have a high raise in the Ohio and still not have a great rain-fall. In the last two seasons I observed we had six inches more water in the Allegheny

than during the five years previous. I observed particularly the great quantity of water in the Allegheny. I think that we will have floods, as yet we have no control of the floods, we can place our buildings sufficiently high to be out of danger.

MR. BROWNE:—Relative to the planting of those trees in Egypt, the reason was this, as I understand it, that the vapor is held in a state of suspension in the atmosphere by the repulsive properties of electricity; that the points or boughs of trees attract the electricity and allow the small particles of vapor to unite and form rain-drops. Now, if this be true, and that it increases the rain-fall in Egypt from six to forty inches, why should it not increase the rain-fall here by planting forests?

MR. METCALF:—This is certainly a very valuable and interesting paper and a very valuable and interesting discussion. There is one point which has not been brought out, and that is the very remarkable formation of the Ohio Valley itself. If any one will take the map and look at it he will see that from near Lake Erie around into Indiana and Illinois, then back into Ohio, New York, and then away into Pennsylvania, then following down the great Appalachian range into Alabama and Mississippi, he will find that the whole country drains into the Ohio river. If we have heavy snows, as they are always liable to come, exceptionally heavy snows and they are followed by exceptionally warm waves, bringing warm winds and heavy rains up from the Gulf of Mexico, we will certainly have such floods as long as the Ohio Valley continues to lie as it does and drains so much area. There is one little thing I made a note of here in regard to railroads. I believe it is a fact in Colorado and other places where these railroads are laid over the plains that the storms which before scarcely ever left the mountains, now follow the railroad track for hundreds of miles, and probably this is due to electricity. I also noticed a fact that occurred at our works during the flood. I observed in taking the high water marks that out in the open yard we had a certain level, which I gauged in different places and found it to be pretty nearly the same, within a few hundredths of an inch. But inside of a new building we had erected, where we had dug a pit for a foundation in the ash bank, and which was in direct connection with the river, I found that we had two or three inches higher water than any where in the open yard. It struck me that the difference represented the difference between the level of the centre of the flood in the river and the lower water in the yard, which was so well observed. I do not know whether that observation is correct or not, but it remains a fact that the water did raise some two inches higher in the pit than in the yard around. It seems clear we can gather from the discussion, that if we had all of the data and knew all of the facts, we could make provision against these floods, but it will be a long time before we can get them. In the meantime, as long as the great rivers from the South pour their waters into the Ohio near its mouth, to check the outflow of the water from the northern and eastern water-shed, the only safety from floods will be obtained by building above them.

STEAM BOILERS, THEIR CONSTRUCTION, SETTING AND MANAGEMENT.

BY L. C. BURWELL.

A paper read before the Engineers' Society of Western Pennsylvania, April 15, 1884.]

As a rule the fireman, engineer, and manager of our various factories and mills, conclude when they hear of, or accidently see, an article written on this subject that it is not worthy of consideration, and in their opinion it is at best but the vain imaginings of some wordy theorist which looks well in print, but bears no comparison in knowledge to "*our* daily practice." This feeling of self-sufficiency stands in the way, and has and will prevent a fair and candid consideration of this subject, which would undoubtedly result in relieving us of a large amount of fear, and give us a much better comprehension of this important institution of civilization. Notwithstanding this condition of things we propose in this paper to discuss this matter fearlessly and independently trusting in part to developments of the future to justify us. We hazard but little in saying that with our reputed authorities there is a vast disparity between their views and practice.

MATERIAL FOR CONSTRUCTION.

The steam boiler is expected to successfully resist a heavy strain, the action of fire and water, changes in temperature and corrosion. The question naturally arises, which of these different points is the most important? We are aware that in general the strain or pressure is almost the only point considered necessary to guard very closely, and as a consequence the great effort in construction is concentrated upon this point, material of high tensile strength, plates of extra thickness, and elaborate staying and bracing are imagined to be absolutely necessary to produce a safe boiler—one that will not explode—and yet while it is thought that the margin of safety is increased you have in reality gone quite a distance on the road towards insecurity, you have obtained high tensile strength at a loss of ductility, which with the heavy plates presents a condition of things not favorable to resist the action of fire, consequently such a boiler will naturally give way under the operation of

the law of expansion and contraction,—by reason of changes in temperature the plates will crack, repairs will be necessary, and it will be a matter of good luck more than wisdom in construction if an explosion does not follow.

We find in fact, as well as theory, that expansion is the most important element in this matter, therefore we should exchange high tensile strength for ductility and heavy plates for lighter ones, it has already been demonstrated that steel plates below 60,000 pounds are better than those above, and here the writer would endorse the position taken by A. E. Hunt as expressed in his valuable paper on "Some of the Properties of Steel," read before this Society a few months since, and will further say that if we can obtain an increased percentage of ductility it would be best to decrease the tensile strength to 50,000 pounds. This with plates 0.23″ thick for a 42″ shell, and 0.35″ for 60″ shell would provide ample margin as against pressure, and largely increase the margin of safety in the other direction, at the same time lessening the first cost and securing greater economy in its operations, especially in the way of repairs.

MECHANICAL PART OF CONSTRUCTION.

We do not propose to go into details on this point. There is no necessity for argument as to whether work should be well done or not, but we wish to say that the parts should be *fitted* together, not forced or drawn together; rivets set fair in their holes and squarely held, then it matters not whether driven by hand or machine, a good job is assured. The different rings in the shell should be fitted together so as to bring the longitudinal seams well up near the top and out of fire limits.

FORM OF CONSTRUCTION.

It is well known that there is great variety of opinions on this point, and as a general thing these opininos are based on a very poor foundation and are valuable only to the owner of them. Ordinarily the opinion of one man is as good as any other, their value is to be determined by the source or principles from which they are evolved. This question of form seems to run in families, corporations and localities. We find some with whom the old plain cylinder is a favorite, because their grandfather successfully prosecuted a limited amount of business with it. The railroad man thinks the locomotive boiler the only thing, and it is quite probable that if his wife did not govern the household he would insist upon running it with a locomotive.

The Ohio River man and the Pittsburgher, well, you might as well undertake to convince him that a basswood chip would make a good circular saw as that a steamboat or rolling-mill could be run with anything but a two-flue boiler. We have merely mentioned these three classes that you might have some data from which to estimate the value of opinions. The plain cylinder boiler should be laid aside as not suitable for any purpose whatever, it is the most dangerous and expensive boiler in use, and for the following reasons: The shell has to receive the direct action of the fire; it has no flues to assist in the generation of steam; all the

units of heat must pass through the shell, and unaided by flues or stays to support it the shell must submit to expansion and contraction, consequently we find them habitually cracking at the circular seams, and frequently tearing apart, causing an explosion with disastrous results.

LOCOMOTIVE BOILER.

We are not prepared to discuss the locomotive boiler extensively, but are disposed to say that it is undoubtedly the best yet constructed for that purpose, but its merits for other purposes may well be questioned.

CYLINDER TUBULAR UPRIGHT BOILER.

This form of boiler has no just claim for favor except one, and that is to use in a place where you have not sufficient room for other kinds, or for portables.

CYLINDER TWO-FLUE BOILER.

For general utility and safety we must commend this style of boiler. It is very strong by reason of its form and method of construction and its steam-producing abilities are of high order, which is owing to the relation existing between the fire surface and quantity of water it contains, yet this boiler is not so perfect in form but what we may call it in question, and for most purposes improve upon it.

CYLINDER TUBULAR BOILER.

This form of boiler more nearly combines all the principles and laws governing the generation of steam than any other, and also the elements of strength and durability, but in its construction due regard must be had to proportions and kind of fuel to be used. If the tubes are small and too many of them, or the boiler too long, it cannot be successful; the small tubes soon fill up, the draft is obstructed and the water space limited. It would seem that with many persons the prevailing idea is the more tubes the more steam and seem to be unconscious of the fact that water is a valuable factor in the production of steam. Any attempt to put cold water in at one end of a boiler and take it out at the other in the form of steam at the same moment, must as a rule prove a failure. The locomotive comes very near to accomplishing this, but at a fearful expense in fuel. This constructing a tubular boiler with only one idea, that of fire surface, produces unsatisfactory results, and has caused many persons to give a sweeping condemnation to all tubular boilers. The exact ratio which the quantity of water should bear to the heating surface I am unable to state, as I have never seen any records of any trials had to demonstrate it, but this much I can say advisedly, that with a tubular boiler 14′ long, 60″ diameter, and 46 4″ tubes, more power can be developed, and steam more promptly delivered and with less fuel than one with the same dimensions and 110 3″ tubes.

ATTACHMENTS AND CONNECTIONS.

As a rule steam drums and domes are utterly useless, and worse than useless. Instead of accomplishing what is desired and expected of them, viz., furnishing dry steam, the reverse is the fact. They must, in obedience to natural law, operate as a condenser rather than a store room in which to dry steam. "But," says one, "I know of a boiler that had a small dome, and it delivered water with the steam constantly. The small dome was replaced with a large one, when the difficulty ceased, and how are you going to get around that?" I answer, increase the cubic contents of the boiler to embrace the contents of the dome, and you will have the same result. I don't wish to be understood as saying that no boiler needs a dome, but it is a misfortune to construct a boiler that does need one. If all the domes and drums in this country were cast aside and their equivalent put into boilers, it would make a difference in the balance sheet, and on the right side of several thousand dollars every year.

MUD DRUMS.

As a rule there is no utility in a mud drum. There may be places where they would be beneficial, but no steam user is justified in using a steam boiler so as to make their ostensible purpose of any value. It would be much better to make them all up into boilers and you would gather dollars, where now you throw them away, except what the boiler-maker gets for repairs.

BOILER SETTINGS AND MANAGEMENT.

Every boiler should be set in a good substantial manner, with ample room, so that every part of the fire surface can be easily reached for inspection. Bridges should only be high enough to keep the fuel on the grate-bars, and in no case should they be built to conform with the circumference of the boiler. Length of grate bars should be governed by the amount of work you propose to get out of the boiler; 4′ is long enough if you desire to treat the boiler fairly, but if you propose to abuse it and enrich the coal dealer, put them in 5′ 6″ or 6′. All fronts should be supplied with doors to the ash-pit, that they may be closed when steam is up in excess of the demand. Furnace doors should never be opened, except for stoking. Open up flue-caps instead. Every battery of boilers should have a good reliable steam gauge, and where several batteries are connected on the same steam line, the firing should be done so as to keep pressure as near even as possible, unless it be desirable to work one hard to the relief of others. All boilers should be kept clean internally and externally. The day is not far distant when a good boiler attendant will be appreciated.

BOILER CAPACITY.

There is more money to be made by the steam-user in the truthful consideration of this point than almost any of the others. It is almost

universally presumed that if we can by crowding make a 60-horse power boiler do the work of 100-horse power, we have gained both in original outlay and economy in daily use, but in this case the reverse is true. As the rating now is to provide 100 where it is supposed that 60 is demanded and continue that proportion in the construction of all boiler plants and our boiler practice would be perceptibly improved, the margin of safety largely increased, repair bills diminished, coal bills lessened, the now dense clouds of smoke dissipated, and general good feeling among all concerned will prevail.

EXPLOSION OF THE BOILER OF THE LOCOMOTIVE DRAWING THE LIMITED EXPRESS ON THE FORT WAYNE ROAD, ON THE MORNING OF MARCH 20, 1884.

Examined this boiler in the yard of the Allegheny shops on Monday, March 24, 1884. Found an extensive rupture in the right side, having its origin in the angle of the throat-sheet extending from there into the seam joining the wagon top to the cylinder part of the boiler, and running to about the centre of boiler vertically, it had also extended in the opposite direction down into the fire-box leg, about an equal distance, making the rupture about 28″ long and opening at the widest place about 6″. This had the effect of stripping four stay bolts in the side of the fire-box immediately adjoining the rupture, and resulted in stripping or breaking every stay-bolt in the right-hand side of the fire-box. Thus freed from its fastenings, the right side of the fire-box was thrown violently against the left, carrying with it part of the tube sheet and doubling down the crown-sheet. At the same time the force of the explosion tore up the track and lifted the rear end of the locomotive, turning it completely around, throwing the engineer and fireman high in the air and forward of the train, killing both instantly. The rupture having produced the explosion, if we can determine what produced the rupture we have a complete solution of the matter. Now, what are the facts? We find that this boiler was built at the Allegheny shops and put into service in January, 1880. Some time after a crack developed in the angle of the throat-sheet, in the same place on both sides of the boiler. These were repaired by putting copper plates over the cracks and fastened with screw rivets. The development of weakness at this point in a locomotive boiler is a more serious matter than is generally supposed, especially with those of the low water school, who would conclude that nothing but a leak would obtain under any circumstances—and for the following reasons: The principal support of these boilers is at each end, therefore this angle, from its peculiar position, is the hinging point of vibration, which may, at times, be very considerable. It follows, then, that this point should be made very strong, and the strength at all times fully maintained. This cracking was evidence of poor material, and examination proves it to be such. It is reasonable to conclude that its inferiority was organic. It may have been injured somewhat in flanging, but if material will not stand the manipulation necessary for construction, it is quite evident it should not be used. To one who understands and comprehends the fact that explosions are the

consequence of defective boilers, or, in other words, that a rupture of sufficient magnitude will produce an explosion, this development of weakness would have moved him to have the entire sheet cut out and replaced with a perfect one, and if this had been done instead of patching this accident would not have occurred.

Who then is responsible for this accident? The answer is: every man who supports the low-water and gas theory of explosions, and the degree of responsibility is in ratio with the amount of influence his opinion exerts.

DISCUSSION,

Mr. Jones: I would like to ask you what you would consider a fair pressure for a 60″ boiler, with 4″ tubes?

Mr. Burwell: Such a boiler properly constructed, the popular opinion in regard to pressure would say that it could safely be brought up to 140 pounds.

Mr. Jones: What would you recommend in that class of boiler?

Mr. Burwell: That would depend upon the boiler capacity provided for a certain amount of business; my individual opinion is that if 100-horse power is provided, where as is now considered, 50-horse power would do the work, then a boiler of size you mention would safely carry 150 pounds high pressure and rapid or excessive evaporation should never co-exist in running a steam boiler. As boilers are generally run we would recommend a pressure of from 60 to 80 pounds, but under favorable conditions our company would insure up to 120 pounds.

Mr. Jones: I would like you to give an opinion of the Babcock-Wilcox boiler, your opinion of the Galloway boiler, and your opinion of the Lancaster boiler.

Mr. Burwell: I ought not to particularize in regard to matters too closely because it might perhaps be misconstrued and bear improperly upon interested parties. So far as I am individually concerned, I have no hesitation in expressing an opinion, but at the same time I wish to have due regard to persons interested.

But I have this to report in regard to that matter, that all sectional boilers are defective from their lack of water capacity; when you have got a certain amount of work out of them and you want a little more you can't get it.

It is different with a boiler of this construction (pointing to drawing) by reason of the large amount of water capacity it has; when you have the water you have a reserve force there, and that must be the condition in all cases, if you maintain equality of pressure when you are running.

Now, if that be the case it follows per consequence that every ounce of water is in condition to go into steam and all that is wanted is the opportunity, and that is why that when a rupture of sufficient magnitude occurs it does go into steam. So if we will construct our boilers and run them so as to prevent ruptures we will have boilers safe to sit by.

By a Member: In regard to boiler explosions, is it your opinion that a boiler could be exploded without an actual rupture, exploding by opening on a long line of pipe?

Mr. Burwell: You can see, philosophically, it makes no difference where the opening is or how the pressure is relieved so that it is relieved effectually. The condition exists the same. If your boiler is perfectly sound in all parts you will be perfectly safe in opening upon the steam line; but if your boiler should be weak, then 3 or 4 pounds of additional pressure at its weakest point would produce an explosion. This sudden opening up will produce a commotion in the water, and it is sure as can be the moment you open up this valve you cause a commotion which brings a strain on the boiler so that if your boiler is weak you will have an explosion, and the weakness is where the danger comes in.

By a Member: The question was suggested by a boiler explosion which occurred about a year ago and that question was brought up in the investigation: it was advanced as a theory of the explosion, as in that instance the line of pipe was a short one.

Mr. Burwell: In all such circumstances you will find it will be a wrong decision to make to attribute that explosion to the opening up of the throttle valve, because the boiler was too weak. Now, as I said before if you will maintain your margin of strength, these boilers are constructed so that the bursting strain is 500 pounds and you run them only to 80, 90 and 100 pounds, very much below their ultimate strength, you have lots of margin and you can do anything with them, even to increasing the pressure a large number of pounds and be safe, and I do not think it should be attributed to the opening of the throttle valve, there has only been a weak point.

By a Member: That is inconsistent with the remark you made before in regard to the rupture, that the releasing of the pressure of steam would rend asunder any material. The release would be so sudden as to cause an immense pressure far beyond that which you figure upon, and there would be produced an explosion. Otherwise the boiler would be entirely safe.

Mr. Burwell: The difficulty is to determine the size of a rupture necessary to produce an explosion, but I think the actual fact in the case was that the boiler was weak. Now in those tests made by Mr. Lawson at Munhall farm, he ran that boiler up to 325 pounds pressure, and yet finally when the pressure was under 300 pounds, upon the sudden opening of a valve an explosion was produced, but there had to be a rupture before the explosion could be secured. The sudden opening of the valve produced an increase of pressure and it gave way at its weakest point, which gave a further relief to the pressure; it tore it all to pieces; tore it to remnants, and yet it exploded at only 235 pounds pressure.

Mr. Roberts: I forget whether I was a high or low water man at the time this question was discussed before, but I recollect I asked a question at that time which was not answered. I would like to ask Mr. Burwell in regard to it. I was on a boat coming up the Ohio river when the safety fuse that was in the flue melted and let out all the steam in the course of about two minutes—I think not longer than that—from over 120 pounds to nothing, and yet all the water did not go out of the boiler. I cannot understand it. The pump was not working, had not been working for half an hour and the water was within 5″ of the top of the flues when we looked into it afterwards.

Mr. Burwell: The blowing out or melting out of the safety plug in the top flue of the boiler of course would naturally stop the man from firing. The consequence is he could not keep his fire up, and would not keep the same temperature there all the time, and when the safety plug blew out or melted out everything was done to relieve the boiler and the temperature of the water was lessened by reason of the escape of units of heat in the form of steam. It was due to the small hole it had to go through that there was no explosion. In the case of this explosion (referring to the locomotive boiler explosion) the rupture was 28″ long and opened out 6″. A hole 1″ in diameter will not produce an explosion, nor 2, 4 or 6 inches, perhaps, but you see the rupture must be of sufficient magnitude. It has been almost impossible to determine what dimensions must be in order to produce an explosion.

The explosion of this locomotive is a very good thing and to me a very interesting study, because I had some opportunity to form an idea of the capacity a rupture must have to produce an explosion.

Mr. Roberts: I cannot understand how the water in my steamboat boiler could come down from 300° to less than 212° in the course of two minutes. I never could understand it.

Mr. Burwell: If you found the water there it is proof positive it could do it. It is probable your estimate of time is deficient.

Mr. Jarboe: (Handing specimen). Here is a piece of metal taken from a boiler 54″ in diameter, 24′ long, carrying 110 pounds of steam, just as it was the Saturday night before they took it out. That place there (referring to corroded part) was over 2′ long. Why did not that explode under 110 pounds?

Mr. Burwell: The surface of the corrosion in that boiler was probably not very great. It was in length, but not in breadth. Well I suppose it is well known that you can take a very thin shell of iron, for instance, $\frac{1}{16}$ of an inch thick, and make it so you can hold water, and you can put 120 pounds pressure and still not burst it, and that is the case in these things. Now, then, this boiler needed something more than that pressure to start this crack, because 110 pounds of pressure might be carried on it. If this boiler was fired up the action of the fire together with the pressure of the steam might start the crack and the pressure will carry the crack still further. This condition of things we find in a great many cases. There are more things of this kind around the country than you have any idea of. Lots of them.

Mr. Jarboe: This boiler was fired very hard.

Mr. Burwell: Yes, I have no doubt of it. The reason a boiler does not explode under these circumstances is hard to explain. The fact that it does stand it is illustrated by the fact that you may take a very weak chain sometimes and you can make it lift a very heavy weight. but still it is not a good thing to practice that. So in regard to steam boilers. They have a good many peculiarities, all of which must be taken into consideration in the handling of them. The only reason this particular boiler (referring to that from which the piece was taken) did not break was because it had sufficient strength to stand 110 pounds, but the last straw was laid on it, and it gave way finally.

Mr. Browne: There is just one word I wish to say, and that is this:

The Pennsylvania Company have been called in question for employing ignorant men. I do not know that there are any men in the country who are more up to the requirements of the times than those in the employ of that company; they are not fogies; they do not possess fogy ideas; they are mechanics and understand their business and all conceivable precautions are taken to render everything perfectly safe, and I think remarks of that kind are not justified.

Now, I do not know where Mr. Burwell obtained such accurate information as to the why and wherefore of that boiler explosion. He did not obtain it from authorized agents of the company, because the men who could give it, would not, and those who did give it necessarily knew but little about it. The facts (?) he states must be based on a vague and erroneous hypothesis.

Mr. Burwell: I suppose that calls for remarks from me, does it not I do not know that I have censured anybody. I am not aware of it. It is pretty generally conceded in these things that a man works up from the basis of his intelligence, and that he cannot be expected to go any farther. It is expected that the man who is raised in the Presbyterian school will preach the Presbyterian doctrine. If he does not do it, of course he will have to seek some other field of labor. It has been so heretofore and is so now. The only fact in this connection I have mentioned, is that the man who believes in the low water theory is incompetent to construct and run a steam boiler, and the only reflection I have made is this, that the parties who had that boiler in charge were students of the low water school. If there is anything wrong in making that charge I am willing to stand corrected. I think that is every practical bit of censure that could possibly be worked out of the case whatever. I know they were low water men, because the master mechanic of the road told me so. The master mechanic had the charge of the shops in which that boiler was constructed.

Mr. Browne: A direct charge is one thing, but a charge by inuendo is meaner than a direct charge. That the company or its employés were aware of the fact that "that boiler was patched up in a way in which no mechanic would do," however, is a direct charge of ignorance, and also implies that Mr. Burwell possesses a very superior knowledge of mechanics— incomparably superior to Pennsylvania Company mechanics at least. As the laudation is self-applied, a considerable discount is admissible.

Mr. Burwell: I insist upon gentlemen taking what I say and not what their imagination may deduce from it.

Mr. Browne: In this case it does not make a particle of difference to me, having no personal interest in the defense, but I think reasoning based simply upon what *I think* and what any one else *thinks*, except it has been demonstrated beyond doubt is faulty. Nothing has been demonstrated in this case, we are simply asked to receive an abstract theory as an axiom. In anything of this kind I do not think one man's theory is better than another's. We should all be placed about equal in matters where proof is impossible or not produced. Demonstration and assumption of a theory are two very different things and should not be confounded.

Anyone must admit that there can be no explosion without a rupture. But what causes the rupture? Who ever saw an explosion without a rup-

ture? Mr. Lawson, at Munhall Farm, took two new boilers—they were supposed to be perfectly sound in all their parts, and of course to explode them it was necessary to produce rupture. You cannot explode a boiler without it. It is an accident to the explosion, the assumption of a rupture.

Mr. Burwell: I am happy Mr. Browne has gotten along so far. Mr. Browne says everybody admits that. I have been travelling and talking with thousands of people, and out of those thousands I have not found three that would accept that theory, that a rupture was necessary to produce an explosion. Perhaps, however, my sensibilities are not as fine as those of the Pennsylvania Company.

Mr. Browne: The only reason why I have said so much on this subject is that there are gentlemen here who are connected with that company, and better able to defend it, but who are too modest to compliment themselves by making a defense and so I felt like taking up the gauntlet. Still I think the name "Pennsylvania Company" is the strongest defense.

Mr. Burwell: I want to speak to-night of the relation existing between myself and the master mechanic in regard to the examination of this boiler. I went down to the Allegheny shops when the boiler was brought into the yard. I went and found the master mechanic in his office. I told him who I was and what I wanted. He treated me very courteously, and we went out and examined the boiler together. After making an examination I had some talk with the master mechanic in regard to the causes of boiler explosions in general. I then left, but went back the next day again and made a further examination. I wish to say that we do not make a superficial examination of these matters. We make a thorough examination, and then take it under consideration, and the result of these considerations will probably make it necessary for a second visit and so on. I have been to the same boiler as often as six times in perfecting an examination in regard to the explosion.

On my second visit back with the master mechanic in talking about that boiler, I told him what was the cause of the explosion, and from that a conversation grew out which was continued to a good length of time, and in which I gave to the master mechanic explanations of our views regarding explosions. He said to me: "I have been in this business for 30 years, and I have been trained to believe that nothing but low water, the condition of low water, would produce an explosion, and while you give excellent reasons, which I am not able to refute, you will excuse me but it is very hard for me to give up my preconceived opinions." That is just what he said.

He suggested I might be a little mistaken about the material, and I said, "If you will get a hammer we will examine it." Mind you, I had not touched it with a hammer to test its condition at all. So we went into the shop, got a hammer, and then we went out there and I got inside the firebox. A piece of that material about as big as my hand was still attached to the boiler. I struck at it and my hammer glanced. It was a kind of a side blow, but on the second trial I got a good fair blow and hit that piece and it broke off like an icicle; it fell at the feet of the master mechanic. I climbed out, and he had picked it up. It surprised him a good deal, and I said, "I guess that was not good material; it don't show up in the con-

struction." He said that "it was quite likely." It was a piece right out of the throat sheet. I took that piece and laid it on an old fire-box and struck it with the hammer. I expected to break it with the hammer, and on the fourth or fifth blow I broke it right square in two. I gave the master mechanic one piece, and I have the other. That shows that a piece of boiler steel, ⅜″ thick that could be broken that easily is not a fit piece to occupy a position of that kind.

I say the only trouble in the whole business is just this—that the gentlemen were raised in the low water school, believe in the low water school, and work consistently for men in that school. There are good men and good mechanics in that school. If you believe in that school, if that is your religion, stick to it. But you should not complain if called upon to assume the consequences.

Mr. Browne: Mr. Parkin has not charge of the locomotives. He does the building and mechanical work, and the road foreman of engines superintends the boilers.

Mr. Burwell: I was not aware that the Pennsylvania Company or Mr. Browne either had reached the perfection point, beyond which it is presumption most base to dare to tread. Yet after all it might be well for Mr. Browne to bear in mind that important ideas have often originated with persons in comparative obscurity, but they were none the less true because of their obscure birth. I would say to Mr. Browne that it is not our custom to have other persons tell us the cause of an explosion. It is our business to determine that matter, and having done so we do not shrink from the responsibility that attaches, neither do we ask for any special pleadings to cover our errors because of our eminent respectability.

Mr. Metcalf: I would like to meet a few of the statements of Mr. Burwell. The fact that steel worked the way the gentleman says it did only proves that the people who were trained in the low water school had also worked in a high heat school, and they burned that plate. No such piece of steel of the quality or strength as the gentleman mentions there could ever come out of the mill; it could not be rolled or worked in any way at all. Whoever flanged that throat sheet burnt it and then sent it out on the road, and the low or high water theory had nothing to do with it, whatever you may say, but it could never have been such a piece of steel as that in the sheet.

Now, the gentleman has made some statements to-night that startled me a little. I have been very much interested in the paper, and learned a good deal, and some things in his theory have certainly upset some of my notions. In the first place in regard to high tensile strength, he says the steel should not be over 65,000 pounds, and it is better that it should be down to 50,000 pounds in order to give it a high ductility. Now, if the gentleman knew anything about steel, as some steel men have to know about it, he would know that steel of such tensile strength as 50,000 is necessarily rotten, that is, it contains so much silicon and phosphorus.

You should not get steel so low in tensile strength, because it is not the right kind of material for boilers. I can show the gentleman tests in steel running 70,000, 75,000 and up to 87,000 pounds in tensile strength with high ductility, steel that can be welded into tubes, and was used by Morris, Tasker & Co., Philadelphia, for making thousands of boiler flues, and also

used largely by the Pennsylvania Railroad in their boilers. It could be bent double—twisted in any form without the sign of a fracture. The company test of that steel was to take a small piece of it, the ordinary size of a locomotive flue, 6″ long, about 2″ diameter, weld the sides together in a lap weld, then heat the whole piece nearly to white heat, then plunge it into cold water; after this they would set it on end under a hammer and come down with the hammer, and the steel would show no defect in the weld. The highest ductility will give a tensile strength of 65,000, 75,000, and even 80,000 pounds, but it depends upon the quality of your material. I know that as a rule low tensile is preferred to high tensile in boiler steel, but when you get too low in tensile you have a material that is injured in some way by some impurities that ought not to be there. In the above tests the steel was not much over .20 carbon or it would have shown some bad results from that frightful heating and quenching in water, but the ductility could not be injured by any such operation.

Any gentleman who will take the trouble to read carefully the first paper that was ever read before this Society as to "Why Steel Hardens," or take a closer view of this matter he will find that with high carbon steel you get a very decided change of volume for change in temperature, and that is the simple reason why high steel is so much mere liable to crack than low steel because a slight change in volume, shown in the specific gravity tables, and therefore you get a steel that is liable to crack. And I think when the gentleman comes to teach us the properties of boilers and says necessarily 50,000 pounds tensility is better he may be touching on something of which he is not well informed.

He next goes on to say, after having ignored tensile strength, that pressure is all along, around and in the boiler, and then says still further that pressure causes a rupture, and the rupture causes an explosion. That is an extreme argument, to be drawn from his premises.

I had intended to say something about his opinion as to the two-flue boiler, but on this pont I think it better to say nothing, since he has spoken so handsomely of our favorite. About grate-bars I agree fully with what was stated in the paper.

In regard to domes and steam drums I believe what the gentleman has said is true, that if you can make the steam room as much larger by the addition of the volume contained in your steam drum you will accomplish the same purpose, but you can not very well do that in the cylinder boiler unless you enlarge the whole diameter and necessarily increase the fire surface, which will diminish the strength of the boilers. It is certain that the steam drum must cause some condensation. I believe it is a good thing to have a reservoir of steam and draw from it. I have seen it in our own shop, where we have ample boiler capacity to supply certain hammers, if we had good size drums there to make a reservoir for steam then we always have enough steam to run the hammers; without the drums we cannot run. I may be wrong in that theory, but I still believe there is some virtue perhaps not in saving units of heat, but in having a steam reservoir and working from it.

I came here to-night expecting to hear a good deal upon another subject, and that is on the care of boilers, which is one I certainly knew the least about. I remember 16 or 17 years ago, when the association the gen-

tleman represents was in its infancy, a representative came to me, or was referred to me, and wished to insure our boilers. We talked the matter over some time, and finally we agreed to insure. He suggested some few little repairs before they would give their policy. I agreed to that, but when I came to look at the conditions they laid down I came to the conclusion that it would be simply impossible to use the boilers and at the same time use their insurance. Of course boiler insurance was a fine thing but it was certain from the conditions it laid down that we were not going to run those boilers. I thought we had better continue to use them. I am very glad to hear to-night that a different policy is now pursued. Of course Mr. Burwell was not that gentleman. I am very glad to hear that in their mature age they have nothing to say about the care of boilers, and perhaps now they do allow people to use their boilers within reasonable limits.

Mr. Jones: This is a very important question, and one that I think should be fully discussed by the Society. I am very happy to say that I coincide with the main points of Mr. Burwell's paper. While I believe it is possible to make steel for boilers that Mr. Metcalf describes, I am equally sure that few firms making boiler steel would exercise the care and discrimination Mr. Metcalf would do in making steel for his own boilers. Now, I believe with Mr. Burwell, that when we want a good boiler constructed we want to know the character of the steel plates, as well as the character of the firm or workman that makes the boilers. I have no hesitation in saying that I would prefer steel for boilers to show 50,000 pounds tensility in preference to steel that will show 70,000 tensility, always considering that the chemical qualities of the steel must be considered. In my opinion I would state as a general rule for steel plates for boilers that I would require tensility not under 60,000 pounds, and per cent. of elongation not under 25; carbon not to exceed 0.10, manganese not to exceed 0.30, phosphorous to not exceed 0.035, and the lower the better, with silicon and sulphur at the lowest limits possible. I can state as a potitive fact, and state it with pleasure, that to American engineers and manufacturers, belongs the credit of first developing the use of steel in boiler construction, and the first to establish by actual experience what constitutes the best formulæ for boiler steel, and that to-day a score of steel establishments in this country are making a grade of steel to satisfy the most captious inspectors. Another question: while I fully agree with Mr. Burwell that the tubular boiler he has described is one of the best forms of boiler to use, yet in determining what type of boiler I would prefer, I would always want to take into consideration what kind of water is to be used. For illustration: After using for eight years at the Edgar Thomson Steel Works 20 tubular boilers 60″ shell, 15′ long, with 36 4½″ tubes, I have no hesitancy in saying that this form of boiler is not well adapted for our dirty and filthy water, and that after using the two-flue boilers, 48″ shell, 28′ long, with two 16″ flues for the last six years, I find them to give far better results than the tubular boilers have given, and I may add here that at the National Tube Works, at McKeesport, the same type of tubular boilers that I have described were thrown out after less than two years' service, simply because it was impossible to keep them in good order owing to the filthy water used. I will readily admit of the dan-

ger of using long boilers, yet when we have to use exceedingly dirty water I do not consider the tubular the proper form to use, but where water is at all reasonably clean, I would prefer the tubular boiler. We have now in use at the Edgar Thomson Works a battery of 12 boilers, 28′ long, 54″ shell, with two 18″ flues. Particular care has been taken to allow for expansion and contraction, and the weight of boilers carefully adjusted on the walls with good bearing, using rollers so as to diminish friction and relieve boiler from strains. These boilers are giving us the very best results, are easily cleaned, and do not deposit or retain as much mud as the tubular. These boilers have no dome, but have large steam space in the shell. We use dry pipes running lengthwise of the boilers, with slits cut at intervals on top of part of dry pipe which is directly connected to steam pipe. All rolling mill engineers know the danger from running engines where the load may vary in a few seconds from 150 to 1,000-horse power, and the constant danger from water in starting engines. To avoid this difficulty all new boilers are so arranged that while engines are standing all condensed water in pipes naturally runs back to boilers, and close to our large engine is placed a steam separator (being a shell or vertical boiler or receiver) 60″ in diameter and 14′ high. The steam is admitted at the top and carried to within 24″ of the bottom of separator while the steam for the engine is taken from the side and near the top of the separator. The separator or receiver is well covered, and there is slight loss from condensation, while we have an almost sure guarantee from knocking out heads or from broken pistons caused by condensed water. I must also agree with Mr. Burwell on the question of thick sheets and immense rigidity in boiler construction, and it is not long ago that I was subjected to severe criticism by boiler makers in not adopting their views of thick plates and many sheets in preference to what we wanted—thin sheets and as few joints as possible, and it is with pleasure that I find my views so well sustained by Mr. Burwell. Another point—our boiler makers should do less drifting and exercise care in constructing boilers, and I have no hesitation in saying that all steel boilers should at least have the holes reamed to correct damage done to sheets by punching, but at the same time I would always prefer the holes drilled, and if holes on putting sheets together do not correspond, use a reamer, but never use the drift.

The paper of Mr. Burwell is an important one and should be fully discussed by this Society.

I would like to see this discussion continue because it is an important one, and no body of men, outside of newspaper reporters can discuss it so well as this Society.

Mr. Metcalf: I would like to say one word in reply to my friend Jones in regard to his 65,000 pound steel. If you can only eliminate as much as possible the dangerous elements, like manganese, phosphorus, aud sulphur, it is all right. In regard to its ductility if you can get anything that will compare with those tubes I spoke of, you will get a steel that will do all that is required in ductility.

Now, I would like to say one word about the boiler steel maker. I do not make it. Not long ago I was about to give out a contract for some steel for this purpose, and was going to give it to my friend in Cleveland, Mr. W., because it is well known he stands in the front rank at least. Well,

it leaked out in Pittsburgh, and some of our manufacturers protested that it was unfriendly to send it out of the city. I was advised to make a chemical and physical specification, and ask for bids from our Pittsburgh friends. I decided not to make any specifications myself, but to ask the steel makers to send in their bids with their own specifications. They did so, and to prove that the very highest quality of steel may be obtained here at a minimum price, if people will only learn what they want and then require it, I give the following as the mean of all of the specifications received:

The specifications for the steel were about like this—viz., carbon, .10; phosphorus, below .03; sulphur, a trace or below .02; silicon, a trace, and manganese not to exceed .30. Finally, the order was placed right here in Pittsburgh, and, I must say, we got a very fair steel, and more than that, if the purchaser can prove that there is a variation above the limit of $\frac{1}{100}$ he can return that material.

Mr. Jones: Right in the city of Pittsburgh we have a number of firms that make equally as good steel as Mr. W. The directions given by Mr. Metcalf of a steel of about .10 carbon, below .03 in manganese, and the other elements as low as possible, will give a steel not over 55,000 pounds tensility, and the Otis steel does not average over this.

Mr. Burwell: The further these remarks go the better the demonstration of the wisdom of this association. I am willing to admit our friend Metcalf knows more about steel than I do. I said if we could get an increased amount of ductility and decrease the tensility even to 50,000 pounds I believed it would be still better. I know the Otis steel has been run down to 53,000 pounds and it has still maintained a forward position, and an advanced position in the steel market for steam boilers, because Mr. W. has been reducing the tensile strength, and that steel has been used with excellent results. So far as I am concerned in the matter I do not want any 50,000 pound steel unless it can be made ductile.

Mr. F. B. Nimick: Did you try any other piece of that throat-sheet other than the one mentioned?

Mr. Burwell: No, I did not. The master mechanic has cut up the boiler. Somebody has to take the responsibility. I could not tell whether or not the steel in this particular part of the boiler had been injured by the flanging. The master mechanic claims very strenuously that it was not, for if it had been the parties using it would have known of it, and they would have had no interest in putting in a defective sheet. I only say this in the way of explanation. I only give the facts as I found them, and let them rest upon whose shoulders they will. I said that the responsibility of this explosion rests upon the advocates of the low water theory, and the responsibility was in proportion to the amount of influence they exert. If that does not distribute it I do not know how to distribute it.

Mr. Metcalf: I certainly did not mean to censure Mr. Burwell. I only said what I did in order to justify a house whom I know are sensitive about their reputation. So far as the sheet was concerned those men put in a burnt sheet, for it was to their interest to do so, because if it was known they had burnt it they might have lost their places.

By a Member: Another word in justification of that sheet of steel.

That piece was subjected to a test after the explosion, and it was found that the sheet ruptured when it was strained beyond its elastic limit, and when this happens we do not know what becomes of it.

BY A MEMBER: I would ask Mr. Burwell if he examined the staybolts?

MR. BURWELL: I noticed them particularly, that some were broken midway, some close to sheets, some the threads were stripped, there was some appearance of fracture in some of them, that might have occurred previous to the explosion, but they were very inconsiderable, and there was an entire absence of corroborative indications that the stay-bolts were the cause.

BY A MEMBER: As to their being broken, each bolt is supplied, I believe, to hold 16 square inches of surface. Suppose there were five or six broken in that way I think it would be a dangerous condition. Now, I have no doubt in my mind, even with the rupture at the corner of the throat sheet, whether that would have caused the explosion, I believe the trouble was lower down where the stay-bolts let go at the time. I noticed that the outside part of the sheet was bulged out. I think that, perhaps, the whole trouble was in these stay-bolts. I believe if the boiler had been put to the test it would not have gone out.

MR. JARBOE: A piece of that same boiler was passed through a pair of rolls, cold straightened, and it broke just like glass, I mean the boiler a piece of which was shown; the boiler that stood 110 pounds, not the locomotive boiler. I can show a piece of the steel; the fracture is just like cast-iron. Adjourned.

NATURAL GAS.

INTRODUCTORY.

The American Society of Mechanical Engineers, having decided to hold their semi-annual convention in the City of Pittsburgh, commencing Tuesday, May 20, 1884, and this being the last regular meeting for the season of the Engineers' Society of Western Pennsylvania, and the paper of the evening, viz., the Report of the Committee of the Engineers' Society of Western Pennsylvania, on Natural Gas, being of general interest the Engineers' Society of Western Pennsylvania extended an invitation to the American Society of Mechanical Engineers to meet in joint session. This invitation was accepted. After being called to order, Mr. William Miller, President of the Engineers' Society of Western Pennsylvania, welcomed the visiting engineers, which was replied to by Professor Sweet, President of the American Society of Mechanical Engineers.

ADDRESS OF PRESIDENT MILLER.

Mr. President and Members of the Society of Mechanical Engineers:

It gives me great pleasure to-night, and I account it an honor, to welcome such a body of men to the City of Pittsburgh.

I assure you that the members of the Engineers' Society of Western Pennsylvania will try to make your visit as pleasant and profitable as your short stay will permit.

It has been asked: "Who are the mechanical engineers?" In reply, I say they are men who are thorough masters of their trade; who carry their capital in their heads; who are independent; whose services are always in requisition.

How often we see young men who are endowed with a fine classical education, and yet how often we see failures of the same young men in a business way. But there is no excuse for a first-class mechanic or engineer ever being in such an unfortunate place. A man possessed of a good mechanical or scientific education, who can not successfully carry himself through life, must be composed of very poor material. A good mechanic needs no financial or political friends to help him climb the ladder of success, the friends will seek him.

Gentlemen, our Society is honored to-night in having such a class of men among us. We will try and make it so pleasant that you will not forget old Pittsburgh. I would say, as you are come to transact your business and see Pittsburgh and its surroundings, to go to the top of Mount Washington, and there you will see, on a clear day, one of the most magnificent views imaginable. Away to the left you will see the Allegheny River, which means "clear water." From there our cities are supplied. Its banks for miles are covered with blast furnaces, rolling mills, steel works, etc.

Then turn to the right and behold the Monongahela River, which means "muddy water," and it is seldom clear. From its banks are supplied all the fuel for our cities and to the western country. From its muddy waters is supplied one of the finest brands of whisky, noted all over the land. You will find in Monongahela rye a solace for every woe.

Now, then, gentlemen, if there is any virtue in it, while your headquarters are here you will have a chance to sample and taste for yourselves.

Betwixt these rivers lies one of the busiest hives of industry in the country, and if we have not much of the fine arts, and small mechanical products to show you, you will have the satisfaction of seeing some of the largest and most complete iron, steel and glass works in the country.

I would presume to say to the engineers here to-night that there is more horse-power developed here from steam than in any other equal space in the country.

Gentlemen, we are proud to feel that we have been placed in this part of the world. Nature has endowed us with plenty of coal, iron, oil and gas. The latter is the latest and the one we know least about.

To-night there will be a paper read on natural gas, and we gladly tender to you the use of this meeting so that you can use it as part of your exercises.

Again tendering you a kindly welcome to the "Iron City," I would be pleased if your President will take the chair.

REPLY OF PRESIDENT SWEET.

Mr. President: In behalf of the American Society of Mechanical Engineers, I thank you, and through you the engineers of Western Pennsylvania, for this opportunity to listen to the paper and join in the discussion.

We have all heard of Pittsburgh and her smoke. We have come here, many of us comparative strangers, to see your fires and forges, your furnaces, factories, your manufactories, your masters and your men. We are glad to have the opportunity to meet with your leading men here to-night and we wish to assure you that we shall try to appreciate, as we certainly shall enjoy the excursion that has been provided for us, where we may see your great industries and the work of your industrious population.

I would like to say a word to the members of our own Society, as the most of you who were with us last fall will not fail to notice the difference between the reception we meet to-night and the reception we met with at the Government Academy at West Point. To those gentlemen who do not know to what I refer, I will explain.

During the arrangement of an excursion up the Hudson we were allowed to stop at West Point and view the military academy, but to say that we met with a cool reception would be slandering the truth. We met with no reception whatever. We walked up the hill, and then walked down again. But no military rule could prevent us from looking over that grand panorama of the Hudson and the Highlands.

The authorities at West Point will learn, as Napoleon learned three-quarters of a century ago, that Providence favors the side with the heavy artillery. Pittsburgh, I take it, is on the side of the heaviest artillery, for here it was that the first large guns of modern times were made, and he among others who has done so much to make our visit pleasant, had a head, a heart and two hands in the work, Mr. Metcalf.

If the Government should have the misfortune to again be involved in war, in the darkest day there will come forth another Ericsson—Ericsson whose name is among the most honored of all on our roll of members will come forward and change the darkness into light.

Our Goverment will find, as England has found, that for her arms, her missiles and her armament, she must appeal to her Whitworth, her Armstrong, her Brown & Campbell, and her Palliser.

Our Government will find that, great as is discipline, grand as is strategy, glorious as is heroism, it is the mechanic in the man, whether he be government officer or civilian. It is the mechanic in the man, whether he be a military chieftain or a Pittsburgh workman. It is the mechanic in the man that forges the weapons, that wings the thunder.

REPORT OF A COMMITTEE, ON NATURAL GAS, APPOINTED BY THE SOCIETY, PRESENTED IMMEDIATELY BEFORE THE PITTSBURGH CONVENTION OF THE AMERICAN SOCIETY OF MECHANICAL ENGINEERS, MAY 21, 1884.

To the President and Members of the Engineers' Society of Western Pennsylvania:

GENTLEMEN:—Your committee appointed in January to examine into and report upon the utilization of natural gas, beg leave respectfully to submit the following report:

After prompt organization, we have held a number of meetings, and have visited several prominent manufacturing establishments where natural gas is being used. We have examined into the methods of distributing and regulating the pressure in mains, and the question of municipal control.

We have from the outset taken counsel with the Board of Insurance Underwriters of Allegheny county.

We have to express our grateful acknowledgments to the following gentlemen for courtesy in affording us facilities for the prosecution of our work: President Ford, of the Pittsburgh Plate Glass Works, Creighton Station, Pa.; Messrs. Atwood & McCaffrey, Messrs. Carnegie Bros. & Co., Mr. Howard Morton, Forward Avenue, East Liberty; Mr. Pew, of the Penn Fuel Company; Messrs. Spang, Chalfant & Co., Fuel Gas Co., all of Pittsburgh.

It is now nearly twenty-five years since the first wells drilled into the sand and rocks of Venango county gave origin to the great and steadily increasing petroleum industry, but we have only recently begun to realize that with the petroleum is associated an invisible fuel, which by reason of its calorific power and the variety of its possible applications may yet assume a degree of commercial importance comparable to that of petroleum.

I. Natural gas from Western Pennsylvania is, in most cases, a mixture of more or less complex character. The few investigations published during the past few years tend to show that it is essentially composed of the hydro-carbons of the series known in chemistry as paraffins. In an accompanying table are enumerated some of the leading members of this series.

TABLE SHOWING THE PROPERTIES OF THE CHIEF GASEOUS ELEMENTS OF NATURAL GAS AND INCLUDING SOME OF THE HEAVIER HYDROCARBONS.

Paraffins.	Condition.	Composition.		Specific Gravity of vapor.	Heat Units yielded by 1 lb. in burning.	Cubic feet of air theoretically needed to burn 1 cubic foot of vapor.
		Per Cent. Hydrogen.	Per Cent. Carbon.			
Marsh Gas	Gas..............	25.04	74.96	0.5576	13,370	9.56
Ethane....	Gas..............	20.05	79.95	1.043	12,469	16.74
Propane...	Gas..............	18.22	81.78	1.522	12,145	23.92
Butane....	Gas, liquifies at 34° Fahr.	17.28	82.72	2.007	Not yet experimentally determined for the higher members.	31.10
Pentane...	Liquid, boils at 100° Fahr.	16.71	83.29	2.49		38.28
Hexane....	Liquid, boils at 158° Fahr.	16.32	83.68	2.97		45.45
Heptane...	Liquid, boils at 210° Fahr.	16.04	83.96	3.46		52.63
Octane	Liquid, boils at 255° Fahr.	15.83	84.17	3.94		59.80

From the table it is evident that the members differ in their relative proportions of carbon and hydrogen. The vapors of these hydro-carbons are heavier as the proportion of carbon is greater. The calorific values show the superiority of marsh gas, weight for weight, over all the others. The first three are odorless; among the others the odor is stronger in proportion as the amount of carbon is greater. A remarkable similarity of chemical properties is exhibited by all, and by reason of the strong attraction existing between them, the boiling point of a mixture is always found to be considerably higher than that of its most volatile constituent. They are theoretically the point of departure for the formation of a great num-

TABLE OF ANALYSIS OF NATURAL GAS—FROM VARIOUS SOURCES.

CONSTITUENTS.	1 Petrolia, Canada.	2 West Bloomfield, N. Y.	3 Olean, N. Y.	4 Fredonia, N. Y.	5 Pioneer Run, Venango Co., Pa.	6 Burns' Well, near St. Joe., Butler Co., Pa.	7 Harvey Well, Butler Co., Pa.	8 Cherry Tree, Indiana Co., Pa.	9 Leechburg.	10 Creighton.	11 Penn Fuel Co.'s Well, Murraysville.	12 Fuel Gas Co.'s Well Murraysville.	13 Roger's Gulch, Wirt Co., W. Va.	14 Gas from Marsh Ground.	15 Baku, on the Caspian Sea.	16 Gas occluded in Wigan Cannel Coal.	17 Blower in Coal Mine, South Wales.
Hydrogen	Chiefly marsh gas with ethane and some carbonic acid.	......	......	A mixture of marsh gas, ethane and butane.	Chiefly propane with small quantities of carbonic acid and nitrogen.	6.10	13.50	22.50	4.79	......	Marsh gas with a little carbonic acid.	19.56	Chiefly marsh gas with small quantities of nitrogen and 15.86 per cent. carbonic acid.		0.98	......	......
Marsh Gas		82.41	96.50			75.44	80.11	60.27	89.65	96.34		78.24		47.37	93.09	80.69	95.42
Ethane		......	......			18.12	5.72	6.80	4.39	......		......		......	......	4.75	......
Propane		......	......			trace.	......	......	trace.	......		......		......	......	......	......
Carbonic Acid		10 11	......			0.34	0 66	2.28	0.35	3.64		......		3.10	2.18	6.44	0.60
Carbonic Oxide		......	0.50			trace.	trace.	trace.	0.26	Trace of heavy hydro carbons.		......		......	......	......	......
Nitrogen		4.31	......			......	......	7.32	......			......		49.39	0.49	8.12	3.98
Oxygen		0.23	2.00			......	......	0.83	......			2.20		0.17	......	......	......
"Illuminating Hydrocarbons"		2.94	1.00			......	......	......	0.56			......		......	3.26	......	......
		100.00	100.00			100.00	99 99	100.00	100.00			100.00		100.03	100.00	100.00	100.00
Specific gravity		0.693	0.692			0.6148	0.5119		0.5580	0.5923	0.56						

1. Fouqué, "Comptes Rendes," lxvii p. 1045.
1. H. Wurtz, "Am. Jour. Arts & Sci." (2) xlix p. 336.
3. Robert Young.
4. Fouqué, "Comptes Rendus," lxvii p. 1045.
5. " " " " "
6. S. P. Sadtler, Report L, 2d Geol. Sur. Pa., p. 153.
7. S. P. Sadtler, Report L, 3d Geol. Sur Pa., p. 152.
8. " " " " " " p. 153.
9. " " " " " " "
10. F. C. Phillips.
11. Robert Young.
12. Rogers.
13. Fouqué, "Comptes Rendus," lxvii p. 1045.
14. Bischof's "Chemical Geology," I. p. 730.
15. Bischof's "Chemical Geology," I. p. 730.
16. J. W. Thomas, London "Chem. Society's Journal," 1876 p. 793.
17. Same, 1875, p. 793.

ber of useful compounds, such as alcohol, chloroform, acetic acid, and glycerine, but, on account of serious technical difficulties, due chiefly to their remarkable resistance to ordinary chemical reagents, (paraffin, parum, and affinis), they have never yet been turned to practical account. They are not actively poisonous. It should be stated that many of these paraffins are known to exist in several different modifications, differing especially in boiling points. Hence the list of boiling points above given must be understood as merely including the temperatures at which the typical members of the paraffin series pass from the liquid to the vapor state. It will serve to show that what is a gas or vapor in summer may become a liquid in the winter.

In the lower sand rocks of the oil regions occur probably all the members of the series, the less volatile flowing as petroleum, and the more volatile existing in a state of compression ready to escape through every opening.

Natural gas is then a mixture of the most volatile of these hydrocarbons, carrying various quantities of the vapor of the less volatile compounds. The lightest member, marsh gas, (so called from its constant occurrence among the products of vegetable decay), is the chief element of the gas likely to be supplied to Pittsburgh. In addition to these, hydrogen, carbonic acid, carbonic oxide, oxygen, and nitrogen are found.

It is stated that C_2, H_4, ethelyne, and other hydro-carbons of the series known as olefines occur, but positive evidence upon this point is wanting, except in the extreme north end of the oil field. An accompanying table gives a general view of the composition of gas from a number of wells.

The table illustrates the predominence of marsh gas. Natural gas is usually a little more than one-half as heavy as air. The gas from Sheffield, Warren county, has a specific gravity of 0.45, while that from Pioneer Run, if the analysis of Fouqué is correct, must be about 1.5.

As the gas and oil sands all have a slight dip towards the southwest the gas in the southern part of the region is drawn from rock strata which are higher in the geological series than those yielding the gas in Northern Pennsylvania and New York State. If any attempt at a generalization may be made with the few data at disposal, it appears, therefore, that the deeper strata yield in general a gas of higher specific gravity and illuminating power.

Analytical data covering a greater area of gas-producing territory may in the future throw important light upon the interesting question of the origin of gas and oil. The theory which traces both to the sea-weeds of the ancient Devonian sea, which once covered Western Pennsylvania, has been very generally popular. Exhalations of combustible gas have been frequently met with in other countries, although nowhere in quantity comparable with the prodigious out-flow from the gas wells of Western Pennsylvania.

In the district Tsien-Luon-Tsing, in China, gas is obtained in large quantity from salt-well borings, and is used in boiling down the brine, and also for illuminating and heating purposes. (Comptes. Rendus, Vol. XII, page 667). Some of these borings are 3,000 feet deep, and penetrate carboniferous strata, yielding gas under great

pressure. Many openings have been made with the special view to utilizing the gas.

The escape of gas bubbles, which readily take fire and burn, is a common occurrence in strongly saline mineral springs. In the salt mines at Slatina, in Hungary, natural gas escaping from fissures has been utilized for illuminating the mines. This is an unusual instance in which the active component of the terrible enemy of the mines—fire-damp—has been made into a useful servant. Considerable volumes of combustible gas frequently issue from fissures in the well-known "mud-lumps," which form at the mouth of the Mississippi.

II. Wells drilled for natural gas, outside of the oil regions, are of recent date, with a few exceptions. The wells at New Cumberland, W. Va., have supplied gas for more than twenty years for the manufacture of bricks. The East Liverpool wells have been burning twenty-five years, and are still productive. At Beaver Falls, natural gas has been used for six years in a cutlery works, but lately the gas has failed, presumably on account of the wells becoming filled up with either paraffin wax in the pores of the rock, or with an incrustation of salts of lime and magnesia, as it is said they have never been cleaned out since they were drilled. At Erie so many wells have been drilled to the strata of gas rock that it has become partially exhausted. In the oil regions a gas well was looked on rather as a curse than a blessing, and, as most of the wells produce gas as well as oil, and so many were drilled to the same sand or rock, it soon exhausted the supply.

Our city has the advantage of being able to tap three or four prolific gas belts or fields: The Butler County field, which supplies Spang, Chalfant & Co.; the Bull Creek, or Tarentum field, which struck gas at 1,147 feet depth, and supplies the Pittsburgh Plate Glass Company, Pennsylvania Salt Manufacturing Company, and will supply Richards & Hartly's and Chalinor & Taylor's new glass houses, and Godfrey & Clark's new paper mill. The Murraysville, or Turtle Creek and Lyons' Run field, which tapped the gas at 1,337 feet depth, and supplies the gas for the Acme Gas Company, used by the Edgar Thompson Steel Works; the Fuel Gas Company, who furnish the gas to the several mills and glass houses on the South Side; the Penn Fuel Company, who furnish the Union Iron Mills, Park Brother & Co. Limited, Wilson, Walker & Co., Hussey, Howe & Co., Shoenburger & Co., and many other works in the same neighborhood on the Allegheny River. The belt or field in Washington County, in which the celebrated McGuigan well is, the gas from which is being piped to the South Side. No doubt other prolific fields will be found to produce gas in the near future.

We have records of depths of different wells in different districts which we thought not best to include in this report.

If small wells are struck on the same belt as large ones, and are not sufficiently productive to be utilized, they should be plugged, as they drain the belt to no purpose. The more durable wells tap the gas-productive strata generally at a greater depth than one thousand feet.

It is a common opinion among those versed in the management of gas wells that the outflow is subject to a gradual diminution, tending ultimately to total extinction. Evidence of this is to be found in all parts of

the gas territory, where gas wells have been long in use. In many localities, however, there is reason to think that the gradual falling off of the supply of a well, is due to the choking up of the pipe by a deposit of salt or paraffin, rather than to the failure of the original source. This is notably the case with the Freeport gas wells.

The following historical facts in regard to the wells drilled by Spang, Chalfant & Co., are of interest in this connection:

No. 1. Has been in use nine years, and is still a good well.

No. 2. Four years in use, still blowing, though with diminished force. Its location is three miles distant from any other gas belt.

No. 3. Yield insignificant.

No. 4. Pressure diminished from $1\frac{1}{2}$ pounds to 0 in one week.

No. 5. Failed after four years' use.

No. 6. In use six years; gradually failing.

No. 7. Failed after five years' use.

No. 8. Good yet; drilled in 1883.

No. 9. Dry hole on Anderson farm; struck quick-sand at depth of over 1,100 feet.

No. 10. Was a small well.

No. 11. A good well; gas struck within the past few days.

These wells being all in Butler County, their partial failure may be due to close contiguity to the numerous oil wells of that district by which they have been drained.

These wells have been supplying the mills of Spang, Chalfant & Co. some years with varying success, being able to supply the entire plant at times, and then as the wells failed, and before others could be drilled, the gas supply was sometimes insufficient, and it was therefore either necessary to stop part of the machinery, or return to the use of coal.

III. The number of companies chartered to supply natural gas in Pennsylvania up to Feb. 5, 1884, was 150, representing a capital stock of $2,160,580. Since that date, a large number of new charters have been granted.

IV. Natural gas next to hydrogen, is the most powerful of the gaseous fuels, and if properly applied, one of the most economical, as very nearly its theoretical heating power can be utilized in evaporating water.

It is used for almost all the purposes to which coal is applied, with one notable exception, viz: for smelting ores in blast furnaces, and it is our belief that at no distant day it will be used for this, but not in the present style of furnace.

Being so free from all deleterious elements, notably sulphur, it makes better iron, steel and glass than coal fuel. It makes steam more regularly, as there is no opening of doors, and no blank spaces are left on the grate bars to let cold air in, and when properly arranged, regulates the steam pressure, leaving the man in charge nothing to do but to look after the water, and even that could be accomplished if one cared to trust to such a volatile water tender. Boilers will last longer, and there will be fewer explosions from unequal expansion and contraction, due to cold draughts of air being let in on hot plates.

Gas engines of large size can be built to be driven by natural gas, as in the case of the Otto, and other styles.

For domestic purposes a beautiful fire can be made, dust, ashes and coal carriage avoided; smoke, and the smoked ceilings and walls of Pittsburgh may become things of the past, yet if sold at prices now charged, i. e., 50 cents per thousand cubic feet, it is much more costly than coal, especially if used in grates and stoves constructed for coal. The invention of burners for its more economical consumption in stoves must follow its general introduction.

TABLE SHOWING COMPARATIVE EFFECTS OF DIFFERENT GAS FUELS.

	Heat Units Yielded by 1 Cubic Foot.	Number of Cubic Feet Needed to Evaporate 100 Pounds Water at 212 deg. F.
Hydrogen	183.1	293
Water Gas (from coke)	153.1	351
Blast Furnace Gas	51.8	1038
Carbonic Oxide	178.3	313
Marsh Gas	571.0	93.8

As the introduction of natural gas has been of such recent date in this city, most of its users consume it in such a crude manner that they fail to get its best results, the difficulty being the expense of making the necessary changes in the burning. There is, however, one notable exception among the large consumers, namely the Union Iron Mills of Messrs Carnegie Bros. & Co., where it is being used with economy in Siemens' regenerative furnaces.

An experiment was made to ascertain the value of gas as a fuel in comparison with coal in generating steam, using a tubular boiler of 42 inches diameter, 10 feet long, with 4-inch tubes. It was first fired with selected Youghiogheny coal, broken to about 4-inch cubes, and the furnace was charged in a manner to obtain the best results possible with the stack which was attached to the boiler. Nine pounds of water evaporated to the pound of coal consumed was the best result obtained. The water was measured by two metres, one on the suction, the other on the discharge. The water was fed into a heater at a temperature of from 60° to 62°. The heater was placed in the flue leading from the boiler to the stack in both gas and coal experiments. In making the calculations the standard 76 pound bushel of the Pittsburgh district was used; 684 pounds of water was evaporated per bushel which was 60.90 per cent. of the theoretical value of the coal. When gas was burned under the same boiler, but with a different furnace, and taking a pound of gas to be 23.5 cubic feet, the amount of water evaporated was found to be 20.31 pounds, or 83.40 of the theoretical heat units were utilized. The steam was under the atmospheric pressure, there being a large enough opening to prevent any back pressure: the combustion of both gas and coal was not hurried. It was found that the lower row of tubes could be plugged and the same amount of water could be evaporated with the coal, but with gas by closing all the tubes (on end

next to stack) except enough to get rid of the products of combustion when the pressure on walls of furnace was three ounces and the fire forced to its best it was found that very nearly the same results could be obtained. Hence it was concluded that the most of the work was done on the shell of the boiler. Another experiment was made with the tubes plugged entirely, and a very small opening leading to stack and with an increased pressure on the furnace and of course a different style of burner; the results were nearly the same but the rivets and seams began to suffer, although only the same amount of gas was burned, but not in the same time. The gas required much more air to accomplish complete combustion per pound of fuel than coal. One singular fact was noticed, that is, when the products of combustion showed the smallest amount of carbonic oxide, the best results were not obtained. This was probably due to the fact that the increased heat due to the burning of the carbonic oxide to carbonic acid did not compensate for the loss occasioned by the amount of air that had to be let in to burn it and which air had to be heated to about 1500°. As the air, gas, and water were all accurately measured, the results were considered very nearly correct. Analysis of the gas in the escaping products of combustion were made quite often, only carbonic oxide and carbonic acid being determined.

Natural gas is being extensively used in heating boilers; in most cases by introducing a gas pipe with a row of small holes on its side, the fire space being closed up partly to check excessive draught.

No other data as to evaporative power is at the disposal of the committee, but it is apparent that in none of the boilers seen by us is the method of heating to be regarded as economical. A portion of gas taken from the flue of a 42″×24′ two-flue boiler consuming natural gas was found to contain nitrogen, 85.88, carbonic acid 6.16, and oxygen 7.96, showing that a great excess of air was passing up the chimney, notwithstanding that in this instance more than usual care was taken in the regulation of the draught.

So long as metres are not employed in measuring the volume of gas consumed in manufacturing establishments, it is scarcely probable that owners will study economy in its use. But with an increased demand for natural gas, particularly when its superior heating qualities, and low price as compared with coal are understood, the officials of the supplying companies will doubtless take such action as will prevent the reckless waste of this valuable natural product. If, for instance, as it might be shown by cheap contrivances, easily applied, a factory could be better supplied with only one-third the present consumption of gas, the owners would certainly deem it no hardship if a meter was placed at their establishment, provided rates were not increased. In fact it is most probable that a perfect system of supply will reach many more consumers and with rates much lower than have heretofore been charged. At present the want of method by the companies, forbids as rapid a development of the gas supply as the public wants really require. Heretofore it seems that contracts have been made to supply the gas at rates only a trifle less than the cost of coal, but in the haste to declare dividends the companies seem to forget that by permitting its reckless waste by a few large consumers, they are crippling a resource which would yield better financial results through a more general distribution at more reasonable rates.

ILLUMINATING POWER.

V. The composition of the gas now being brought to Pittsburgh renders it improbable that it will compete with coal gas as an illuminant, until some specially suitable form of burner has been contrived. Pure Marsh gas yields about one-half the light produced by coal gas.

Experiments made with a view to charging natural gas with the vapor of heavy hydrocarbons, have thus far been unsuccessful, the mixture thus far tending to separate in the gas holder into layers of different composition.

USES.

VI. It has been attempted to apply natural gas to the convertion of iron into steel.

Experiments having in view the dephosphorization of iron through the agency of the hydrogen of natural gas have been made, but thus far the results have been very unsatisfactory. Imperfectly burned at a high temperature the gas deposits carbon in a form having a remarkable density. Upon this principle the manufacture of electric light carbons is now becoming an extensive industry in the hands of the McTighe Electric Light Company.

The tendency of the gas when under pressure is to absorb and carry off oil and grease, and leads to its being used for the cleansing of delicate fabrics.

The powerful reducing action of the gas upon metallic oxides at high temperatures may lead to its application to the smelting of metals upon a large scale.

The application of gas to glass making on account of the purity of the fuel has led to the production of superior glass, more rapid fusion is possible, and covered pots are found unnecessary.

VII. Pipes of various sizes and strengths have been tried and with different kinds of sockets or couplings. Standard weight wrought iron pipes with fine and coarse threads, tapering threads and sockets, light pipe with the Converse joint, (which is a cast-iron socket caulked with lead, the same as ordinary cast-iron water pipe).

Lead rings have been used between the beveled ends of the pipe, in the regular socket. Pipes have been screwed together with fine threads, and the sockets caulked with copper wire. Cast-iron gas pipe with caulked lead joints has been used at Wellsburgh, West Virginia, but on account of the high pressure of the gas, proved a failure, the gas leaking not only through the joints, but through the pores of the iron in many places.

The tapering socket with pipe cut to match seems to have the best record.

If standard wrought-iron pipe be used and laid in ditches below the frost line and care taken in laying, no allowance need be made for expansion, for the flow of gas will keep the pipes at a fairly even temperature of not much over 45° Fahr., and no trouble from expansion or contraction need be feared. This statement, of course, not applying to lines laid in cinder banks or where they are exposed to extreme changes in temperature due to proximity to furnaces, etc.

Light oil-well casing should not be used for pipe lines, because first, it is only .1885 inches thick at its thickest part, and the thread (14 to the inch) reduces it .061 inch leaving therefore, only .1165 inch thickness, which is not sufficient. Again, some soils, and more especially cinder banks will rapidly corrode such thin pipes.

The Acme Gas Company use a $\frac{1}{8}$-inch pipe of somewhat less than the standard weight, but still heavy enough to resist all ordinary pressure and strains.

The Fuel Gas Company has two lines of $5\frac{5}{8}$ light casing, but they will never repeat this mistake, as they are about to lay two lines of standard weight pipes; all their city connections are made with standard weight pipes.

The Penn Fuel Company laid one line of $5\frac{5}{8}$ *casing*, and one line of 8-inch *pipe*. Connections to mills are made with standard pipes, but it is to be regretted that this company laid any casing inside the city limits.

The varying requirements of a large iron works will render it desirable to be able at all times to control an unlimited volume of gas supported by high pressure.

In private dwellings the danger from explosions due to leaks in the pipes would be enormously increased by a pressure much exceeding that of ordinary coal gas in the service mains, in the opinion of the committee a pressure of over 6″ water pressure should be forbidden by law in pipes leading to dwellings.

TABLE SHOWING COMPARATIVE INFLAMMABILITY OF NATURAL GAS AND COAL GAS.

Mixture of Natural Gas and Air.		Effects.	Mixture of Allegheny City Coal Gas and Air.		Effects.
Gas.	Air.		Gas.	Air.	
1 volume.	4 volumes.	Burns feebly.	1 volume	4 volumes.	Burns feebly.
1 "	6 "	Burns slowly.	1 "	6 "	*Explodes.*
1 "	9 "	Burns slowly.	1 "	7 "	Burns explosively.
1 "	8 "	Burns rapidly.	1 "	8 "	" "
1 "	9 "	Burns explosively.	1 "	9 "	" "
1 "	10 "	*Explodes.*	1 "	10 "	Burns explosively—less rapid.
1 "	12 "	Burns somewhat explosively.	1 "	12 "	Flashes.
1 "	13½ "	Burns quietly.	1 "	13½ "	No flash.
1 "	15 "	Flashes, but flame dies out.	1 "	15 "	"
1 "	16 "	Very feeble flash.	1 "	16 "	"

The importance of having the high pressure mains, as they enter the city suburbs, subjected to careful tests, and the mode of laying such pipes under municipal control, cannot be over estimated.

We are convinced that most scrupulous care is being bestowed both in the construction of pipes and valves by prominent manufacturers, and in the selection of material by some of the gas companies.

The necessity for a reduction of the pressure, which is often 75 or 100 pounds per square inch as the gas comes from the well, to an amount not exceeding 5 inches water pressure in the street mains, renders the selection of regulating valves for accomplishing this purpose of great importance. The regulators proposed are of two classes:.

1. *Valves.*—Among the best known is the Luther valve, by which it is proposed to reduce the high pressure in the mains leading from the well to an amount suited to the purpose to which the gas is to be applied, and to preserve constantly this lower pressure.

From what the committee have seen in the use of valves, we believe we are justified in the statement that not one has yet been suggested which will satisfactorily answer the purpose.

2. *Tank Governor.*—This is undoubtedly the best form of regulator which the committee has seen tested. It is similar in principle to the gasometers or holders employed at the large gas works in the country.

EXPLOSIBILITY.

VIII. The fact that natural gas if mixed with air will explode on contact with fire, and is in effect the dreaded fire damp of the coal mines is no argument against its introduction and general use under due precautions.

To those who understand its character, it is wholly unnecessary to state that the qualities which render it explosive when mixed with excess of air are the very ones which render it valuable as a producer of light and heat.

Taking the gas from Creighton Station, Western Pennsylvania, as approximately representing in composition the gas now being used in the city, the following trials were made with a view to ascertaining the limits of its inflammability.

Different mixtures of measured quantities of natural gas and air were prepared, and also mixtures in the same proportions of coal gas and air. The effect was noted when a coal gas flame was plunged into each. From these results we concluded that in a room filled with air containing 1/10 to 1/12 gas the danger would be one of explosions; above or below these limits, there would be danger of fire, but not of explosion.

A natural gas charged with the higher members of the series of paraffins (see Table page 332) would flash or explode when diluted with a still larger proportion of air. On the other hand if the air in a room contains 1/6 or 1/7 coal gas contact with flame would cause explosion, while with an admixture of 1/10 or 1/11 of coal gas, there would be danger of fire, but not of explosion; as will be seen as regards safety there is a difference in favor of coal gas.

As coal gas is richer in free hydrogen, the most easily inflammable of all gases, its temperature of ignition may be assumed to be somewhat lower. The well-known property of coal gas of rendering a mass of spongy Pt. or Pd. incandescent is found to be generally wanting in natural gas.

Accurate experiments (Bulletin de la Société Chimique, 1883, page 2, Mallard & LeChatelier) have shown that a mixture of O. and H. ignites at 552° C. while a mixture of Marsh gas and O. ignites at a temperature between 600° and 660° C. A calculation shows that a cubic foot of natural gas mixed with 9.2 cubic feet of air, and fired, will produce an expansion to 91 feet. A cubic foot of coal gas mixed with 6½ cubic feet of air will, on explosion expand to 73.3 cubic feet. It has been found that a flash travels in an explosive mixture of natural gas and air at a rate considerably exceeding 18′ per second in a 2″ pipe.

Natural gas brings with it from the well the minute quantities of heavier liquid or solid hydrocarbons which are carried along in the form of vapor or spray by the force and velocity of the gas under high pressure, and impart to it a strong and characteristic smell.

A peculiar substance resembling butter is often taken from the mains bringing the Murraysville gas to the city. A specimen of this substance was found to contain common salt, water, small quantities of lime and magnesia salts, coarse sand and a considerable quantity of solid paraffin all blown into a kind of light froth.

The odor of the gas in the mains appears to be dependent upon these traces of condensible hydrocarbons, for if kept in a closed vessel for a few days, the gas becomes absolutely odorless. The odor will therefore in all probability diminish more and more as it is carried away from the wells, or from the high pressure mains. This may explain the contradictory statements upon this point which have found circulation.

It has been found that air containing 10 per cent. of Murraysville gas (fresh from the high pressure mains) has a decided odor; this is also true of Freeport and Creighton gas, but the same gas after standing in an air-tight glass for 24 hours had lost every trace of odor. Owing to their minute quantities and rapid condensation, these heavier hydrocarbons are not easily accounted for in an analysis.

Air containing 2 per cent. of Allegheny City coal gas has been found to possess a decided odor.

IX. The velocity of the gas depends largely on the amount of friction it has to overcome, as well as the initial pressure it has in coming from the well. A well which with its conducting pipes indicated pressure of 3¼ ounces of water at the mouth took just 4¼ minutes for the gas to traverse the 16,000 feet of pipe, which was then connected on, the pressure running up to 15 pounds at the well, due to the increased resistance in the friction of the pipe. The following experiment was also tried. Gas was turned into the pipe with an initial pressure of 90 pounds per square inch. It took just 2¼ minutes for it to traverse the 16,000 feet of pipe.

Natural gas pipes should be laid without any right angled elbows, or other fittings of the kind; changed direction in the line should be made by bending the pipes, and no bend should have a radius of less than 48 inches for a 6-inch pipe, or eight times the diameter of the pipe.

Gas from a well having a pressure of 20 ounces had a velocity of 23,400 feet per minute; a rubber ball was driven through three miles of a 5⅝ casing pipe in 2½ minutes.

When gas is blowing freely from the mouth of a well, the pressure has not been found in any case to reach 2 pounds per square inch.

Statements in regard to higher pressures than this are probably in error.

The gas as it issues from the wells has a temperature of 42° to 45° Fahr.

At the moment of release from the well the volume no doubt undergoes a very considerable expansion, resulting in a lowering of the temperature.

This absorption of heat due to expansion may perhaps explain the fact that blocks of ice are often seen to be thrown from the stand pipes while the gas is burning with a powerful flame.

The temperature has been found to be 45° in several of the mains in Pittsburgh.

X. At the time of the appointment of this committee, the prominent legal question of interest to natural gas companies was the definition of their rights to lay their pipes in the city streets, and as corrollary to this, the responsibility resulting from explosions or other accidents due to the use or presence of the gas. But recently a more important question has been brought forward by the decision of the local court that but one natural gas company under the law of 1874, is authorized to supply gas to consumers in this city.

As the law of 1874 is so frequently referred to in the newspapers, it may be useful to introduce it here for the benefit of readers who may not have ready access to a law library.

LAWS OF PENNSYLVANIA.

Act approved 29th of April, 1874, relating to water, gas, light and heat companies.

*1. Companies incorporated under the provisions of this statute for the supply of water to the public, or for the manufacture and supply of gas, or the supply of light or heat to the public by any other means, shall, unless otherwise provided by this act, from the date of the letters patent, creating the same have the powers and be governed, managed and controlled as follows:

Clause 1. *Gas, heat, light—powers of companies.*

2. Where any such company shall be incorporated as a gas company, or company for the supply of heat and light to the public, it shall have authority to supply with gas light the borough, town, city or district where it may be located, and such persons, partnerships and corporations residing therein, or adjacent thereto, as may desire the same at such price as may be agreed upon, and also to make, erect and maintain therein the

1. Act 29th April, 1874, 34 P. L. 93.
2. Id.
3. In Bloomfield, etc. Gas Light Company vs. Calkins, 62, N. Y. 386.
1. Thomps & Co., 541; it was held that gas pipes could not be laid under country highways without compensating the owners of the fee. See supra. p. 56, N. 2, Dillon vs. Gas Light Co. 1 McAr hur, 626.

necessary buildings, machinery and apparatus for manufacturing gas, heat or light from coal or other material and distributing the same with the right to enter upon any public street, lane, alley, or highway, for the purpose of laying down pipes, altering, inspecting and repairing the same, doing as little damage to said streets, lanes, alleys and highways.

3. And impairing the free use thereof as little as possible, and subject to such regulations as the councils of said borough, town, city or district may adopt in regard to grades, or for the protection and convenience of public travel over the same.

When the act of 1874 was passed, electric lighting was in its infancy, and it has been suggested that in deference to the promises of scientific men regarding its then future, that the terms of the act were purposely made so diffuse as to cover its possibilities—as instance, "The supply of gas, or the supply of light or heat to the public *by any other means.*"

This vagueness and want of supplementing legislation is a source of evil in various ways, and it is the opinion of some well informed, that as the knowledge of electric lighting and of natural gas is *now* better understood, there is an urgent demand for additional legislation.

There can be no doubt about the fact that the intention of the lawgivers, was to encourage both the electrician and the gas man, but the wants of the two interests are so different that only confusion and trouble will attend their affairs, until their rights and privileges are separately, and distinctly defined, and this it is reasonable to believe could be much more effectually accomplished by new legislation, than by awaiting decisions based upon such fundamentally defective laws, as that of 1874.

Under the law as understood by this committee, natural gas companies have the right to lay their pipes in the streets of towns and cities, with precisely the same privileges as coal gas companies so far as such rights extend. But this does not prevent cities from passing laws based upon "reasonable grounds" ordaining the mode of laying the pipes as far as the language of the act cited allows as regards grades or for the protection and convenience of public travel over the same.

In regard to the question of the right of the city to restrict the gas companies, as to pressure in, and size and strength of pipes, etc., a difference of opinion exists, but under the head of "police regulations" it is believed by the committee that any insufficiency of strength in the pipes, or defective workmanship in the joints which might cause explosions, a city can in its charter find grounds for interference, and protect itself by the enforcement of appropriate legislation. But from the extent to which even this apparently wise and proper proposition has been debated, it is made clear that further action is demanded by the legislature. It does not, of course, comport with the character of this paper, even if the committee felt able to do so, to attempt to criticise the recent decision of the court, which is generally construed to grant one company, to the exclusion of all others, the privilege of supplying the City of Pittsburgh with natural gas.

Natural gas had been used under the boilers of engines at wells in the northern oil fields before the passage of the act of 1874, and to some extent it had been introduced into dwellings for domestic consumption, but it was then considered a waste product, which any one could dispose of

by sale or gift, as could be done with any natural product, such as coal, limestone, or other minerals. It simply flows from the ground, with no process of manufacture involved in its formation, and no patentable form of composition, force or capability. It would, therefore, be strange indeed if our legislators intended natural gas to come within the scope of statutes governing the organization and specifying the privileges of manufacturing companies.

If the Engineers' Society of Western Pennsylvania were called upon to suggest on what points in connection with the subject of natural gas, legislation was desired, this committee would not hesitate to recommend for consideration, as follows:

First.—That a State Inspector of gas and artesian wells, and natural gas companies be appointed by the Governor, among whose duties we would specify: To report to the proper authorities, after due notice, any waste or extravagant use of natural gas, either at the wells or elsewhere, and that all natural gas companies be required to report to him the details of their operations, force and flow of the gas, and that all arrangements for its control at the wells, or in the pipes to a city, or borough line be under his general supervision. His office being compelled to keep up the records, and to report annually, with the aid of statistics and plans, the state of the natural gas supply and demand.

Second.—That in cities or towns, companies desiring to supply either manufacturers or domestic consumers with natural gas, may with the consent of the councils thereof, and under the direction of the city or borough engineer, lay their pipes through or along any street, lane or alley, but no city or borough to have the right to give any exclusive privileges to any one gas company.

Awaiting the action of the State Legislature on the general matters which it might be expedient for that honorable body to consider such as we have outlined above, your committee feels that they might be considered derelict in duty if they did not advise and propose, at least some measures which might be exercised by individual consumers of natural gas, tending to both security, as regards the dangers of fires and explosions, as well as to economy in its use.

We therefore would propose for the consideration of companies and individuals interested, the following:

First.—That the distributing mains for domestic consumption of natural gas be of size amply sufficient to conduct gas to dwellings with at no time or place a pressure exceeding 5½ inches (water pressure). This pressure can be guaranteed to be uniform, certainly never in excess by a properly constructed form of tank governor.

Second.—Every domestic consumer of natural gas should see to it that an automatic cut-off valve be placed on his service pipe, so arranged in case the supply from the tank governor should from any cause fail, that his valve would immediately close the pipe conducting to his premises, and requiring personal attention to restore the pressure when it again returns through the main. The committee believes that such automatic valves can be provided.

Third.—No cast-iron fittings, or parts of fittings of cast, or any coarse-grained or inferior metal, should be allowed in private houses. Carefully

selected wrought-iron pipes, with brass or malleable iron fittings should alone be adopted. The great object of care being to prevent the possibility of leaks.

Fourth.—Special care should be taken to see that the street cut-off, or valve, is so boxed or tubed as to permit free outlet to the air of any gas escaping from a leaky main which may follow along the branch pipe from the street. The work of trenching to and tapping gas mains should be done when the frost is out of the ground, and the soil next the pipes in such trenches should be clay, well wetted and puddled.

Fifth.—In the case of fires under boilers, in ranges, fire places, stoves, etc., consumers of natural gas have, for the most part, entirely disregarded its laws of combustion in not using the proper appliances for the admixture of air to the gas jets. In many instances, even among wholesale consumers of this fuel, it can be demonstrated that with the use of improved mixers the same quantity of heat can be produced as is now developed, from one-third the volume of gas.

Sixth.—The feed valves to fires, and all burners where natural gas is used in private houses should be securely placed so far above the floors as to be out of the way of children and where they could not be accidently turned on. To this end valve stems with movable socket handles are of advantage.

Seventh.—All gas pipes laid from service mains should be thoroughly tested for leakage before being accepted as in working order.

Eighth.—Where natural gas is sold either entirely or as a component part of illuminating gas its candle power should be guaranteed to be of a satisfactory amount.

Respectfully submitted,

T. P. ROBERTS,
F. C. PHILLIPS,
A. E. HUNT,
W. S. JARBOE,
N. M. McDOWELL,
Committee on Natural Gas.

DISCUSSION OF THE REPORT ON "NATURAL GAS."

PRESIDENT SWEET—This has been a very interesting paper and is now open to discussion. The members of each Society are expected to take part in the discussion.

MR. KENT—It seems that the engineers of Western Pennsylvania and the Mechanical Engineers of the United States are a little afraid of each other to-night. When I was an active member of the Engineers' Society of Western Pennsylvania, our then President, Mr. Metcalf, used to call on me to start the debate on any subject. Now I, as member of both societies, think Mr. Metcalf thinks I must start this discussion, but I hope some others will carry it on. Mr. Metcalf is best capable of talking about gas, I guess, but I may as well, while I am up, say a few words regarding what I have heard in regard to the economy of gas under steam boilers.

Consumers of gas to-day are no doubt comparatively in the dark as to the best method of securing economy of gas. There are a few very simple principles which they should work by in order to secure that best economy.

First, secure the highest possible temperature in the front end of the boiler, and second, secure the lowest possible temperature in the back end of the boiler. The highest possible temperature can be secured in the front of the boiler by mixing the air with the gas with just the amount needed to secure its perfect combustion, and no more. It may be that amount of air is much larger than the amount theoretically needed, as we find in burning coal that nearly double the actual amount of air is required as theoretically must be passed through the coal in order to burn it thoroughly, and secondly, there is nearly twice as much air going up the chimney as there ought to be theoretically.

It may be assumed with natural gas that just so much air should be supplied as will generate the highest possible temperature. After having obtained the highest possible temperature in the front end, in order to obtain the lowest possible temperature in the back end of the boiler all that is necessary is to supply sufficient heating surface to the boilers. Some months ago I visited a place where they were using some natural gas and I was told that whereas before they introduced it the number of boilers they had was too small to do the work required, after they used natural gas they had thrown off several boilers and got along with very much less. The boilers before were over-driven. They had to maintain too high a temperature to obtain perfect combustion, and it was natural to suppose, arising from the temperature, there was a very much greater waste of fuel than when gas was used; too much air passed in, a waste going on which accounted for the boilers doing twice as much as they should have done. The high temperature in the furnace chamber can no doubt be best secured by burners which intimately mix the gas and the air, and by having the combustion chamber thoroughly surrounded by fire brick. There is no limit to the heat short of melting down the fire brick, and, if you have made that heat, then the boiler's business is to absorb it.

I give this rule, therefore, for natural gas: burn the gas with the smallest possible quantity of air which will burn it thoroughly, although that may be more than the theoretical amount, and by means of a fire brick combustion chamber keep the furnace chamber hot, and then keep enough heat on the furnace to absorb the heat and let the gases go off at the lowest possible temperature.

MR. JARBOE—I differ with Mr. Kent as to burning all the gas in the front part of the boiler. After a number of experiments I find the most economical and the best evaporative results are obtained from burning the gas equally along the length of the shell, distributing the supply, putting the flame against the boiler. In one boiler, I had fired the gas sideways across the boiler. It gave me the very best results I had but it was good for the boiler makers. It made the rivets melt and the shell open.

With a furnace running at the present time the boiler is fed with gas from a series of jets, along the whole length of the boiler (the flame from each and every individual jet touching the shell), with no combustion chamber in it. The pipe is placed about 8″ from the boiler and the jets running up in the bed. That boiler has an air tight furnace, so to speak. Instead of having a 36″ stack it has about a 6″ opening to let the gas out. That boiler is doing splendidly. It has given a high result, and the re-

sult is not obtained from theory, but it is given from data taken on the spot. The gas is taken through two meters, of different makes; the air is metered in to get the proper quantity so that I can get proper data from that. The water was metered into the boiler also, and that boiler is running to-day, with jets the whole length of the boiler and no combustion chamber.

Question by a Member—Will you state how many pounds of water were metered in per pound of gas?

Mr. Jarboe—20.30 pounds. That is high.

Question—What is the theoretical limit?

Mr. Jarboe—About 27.8 pounds.

Question—But I understand from the report only one-third of the heat was utilized?

Mr. Jarboe—This boiler has a stack placed 20 odd feet from the boiler and the heat from the boiler is almost nil. There is a piece of tin in the stack, and that piece of tin has never melted yet.

That one-third business you heard of was where they had two flue boilers, five in a battery, 14″ flues, a 36″ stack, 60′ high. No measurement of the air mixed with the gas has ever been taken. The parties said they paid $3 per ton for the iron finished, and the engineer of the gas company had to look out for the economy of fuel.

Mr. Metcalf—I regret very much I have such a cold. I am afraid I can not be heard at all to-night. We came here to-night to hear something about natural gas. I came here a complete ignoramus, not knowing about it, and I feel very much the same way now. I hardly know what you are going to talk about. Almost all of us knew it was mainly marsh gas; that it had high calorific powers; that it did not smell and it did smell, and of course these are all matters that are necessary in a record of this kind, but these gentlemen have not told us anything yet of the value of this gas; there is no comparison between the value of this gas and solid fuel, and no comparison between this gas and the burning of coal, or the different modes of producing gas, or the system of the regenerative process.

I know perfectly well Mr. Jarboe is right. The companies come along and say they will supply us gas for our mill, and of course that is such a nice arrangement at present that a few have taken it up, but the time will come when so many will take this gas that it will be necessary to furnish it at a very low pressure. It will be necessary to expand it in volume to run it in a rational way, like ordinary producer gas. When the time comes to these gentlemen who are offering to supply gas, who is going to guarantee against the stoppage of the works and the loss?

For instance, our friends state that the only place where the gas is utilized now in Siemens furnaces is at the Union Iron Mills. It is also used at the Black Diamond Steel Works. Suppose they have ten furnaces full of heats and the gas suddenly goes out, who is going to pay for that? What security is there in using it now when the records show that the wells peter out in from one day to two or three years?

I would like to know what is the value of this gas and how it can be utilized with that low pressure; what it is worth per thousand cubic feet. will tell you what the coal value is worth, and then we can soon come to

a dicker with these gas companies and we will know whether we are dealing intelligently or not.

We are now puddling iron in regenerative gas furnaces, using slack, for .75 of a pound of coal for the pound of muck iron; $\frac{3}{4}$ pound of good slack. This is done every day; done all the year through, thousands of tons. We also melt steel in crucibles in Siemens furnaces for 1 pound of slack for the pound of steel.

These are figures very easy to get at and carry out in tons. Now the question is how much shall we pay per thousand cubic feet of the gas to justify us in throwing off these furnaces in order to use natural gas. With all due respect to the committee if they had told us more about these things and less about the law I would have felt better informed.

MR. ROBERTS—It occurs to me that I recollect the very first paper that was read before the Society of the Engineers of Western Pennsylvania. It was dedicated to the subject "Why Steel Hardens." That paper I understood was the result of many months of labor and a considerable expenditure of brains, but I think the result was "if any person knows any more on the question why steel hardens" than the author of that paper, he would like to hear about it.

Now, gentlemen, steel is something which has been before the public for, well I do not know how long. I have heard of the Damascus blade and the Toledo blade, and it has been dignified as a metal I do not know how long. But this natural gas question is something entirely new. We are dealing entirely with an invisible fluid and we have presented statistics about it as far as we could obtain them to date for the information of these two honorable societies. Possibly some of the members may have been asleep when this information was given, because this is an invisible fluid and will leak through a man unless his joints are tight. It differs from steel. You can see something in steel when you hammer it out, but this gas has no odor. It is not a tangible material, and I think you have got to study over it very carefully to know just what has been said. Candidly, however, the committee recognize the short comings of their report, and they know that many further examinations will be required to exhaust the subject.

MR. HUNT—I would like to make another correction. It has been stated that the report says that natural gas has not been successfully used in the making of steel. What the report actually did mean to say was that it had not been successfully injected into or otherwise used in the way that the melters call as a "medicine" for steel. We all know in several of the largest steel manufactories of the city it has been successfully and well used as a fuel both in Siemens gas and other forms of furnaces and we have so stated in the report.

MR. JARBOE—The only experiment I know of where the value of gas and coal has been compared is in the shape of the comparative test of fuels. I took coal, Pittsburg selected lump coal, at five cents per bushel, and used the gas with the best arrangement of burners. I found the gas was worth 7 8.10 cents per thousand cubic feet. That is by actual measurement, burning it as well as I could burn it, with the smallest trace of carbonic oxide. That was in the same furnace I mentioned. The air is mixed

with the gas outside of the furnace and injected into it, and no other air can possibly get in.

Mr. Reese—I would like to say that Mr. Beal, of Leechburg, has been using the natural gas in his open hearth furnaces to make open hearth steel for some time. He has discovered an incident connected with the use of natural gas that has not been mentioned and I will take the opportunity of mentioning it.

Natural gas, as our friend there said, is a new thing. The committee has told us that its specific gravity differs from that of air about one-half. Probably the Leechburg gas is .58 or .60. Now it has been discovered that these two elements, gaseous elements, should be put together to give the best results at the same velocity. When the specific gravity varies the velocity will vary and it has been found, where experiments have been made, that where the gas and air were sent through the regenerators together they had nothing like the result, economical or calorific, that they had where the air was sent through the regenerators and the gas was put in cold, the gas being so much lighter its velocity was greater, being put in at a high pressure, that the two coming together it was impossible to secure perfect combustion. You know when you consume anything, there is a relative velocity of combustion. Now to secure this relative velocity and economical combustion, it is necessary that the air and the gas be admixed at the same velocity. This could not be secured by passing both the gas and the air through the regenerators, as the gas was so light that it moved with greater rapidity than air. So that it was found necessary to heat the air alone, and by thus uniting the hot air and cold gas, the relative velocity and perfect combustion was secured. Don't say I have taken out a patent. I have not got it yet.

That is one point that has been determined of very great importance. I would just say another thing in reply to my friend here from abroad, who asks something about the continuity of the thing, whether it would last. I go down to Beaver county occasionally and down at the mouth of Raccoon Creek, three miles below Philipsburg, there is a salt well. They have a pipe running into a big tank. The gas squirts the salt water up and the salt water is run into the tank and the old fellow that runs the well sits down and lets it boil. It boils away. How long will it last? He said he did not know. He has been letting it boil for 21 years and he don't know how long it will boil. Let me see—this is 1884. In 1860, I think, I was interested with another party when we went up and bought a large tract of land in Venango county, Venango and Clarion. We were going up to bore for oil. There was another party there alongside of us who was going to bore and we thought we would wait and see what he struck. Well, he struck gas. We went up there, a lot of us from Pittsburg—John Scott was one of the boys,—they had five wells. They were getting some oil and a good deal of gas. Well, we suggested, some of us that we had better turn the gas under the boilers, and they did so and it worked nicely. It pumped the water and they thought they might as well put it into the cylinder, and it was put in there. This is the way they ran the machine and that is now 23 years and the gas is going yet and there is more gas now than there was at that time, and there will be more if they bore another well.

I want to say one word to my friend here that read the paper. He says that it will dephosphorize. That it can be used in the smelting of iron but not in the present blast furnaces. Now, I do not see how 96 per cent. of marsh gas with 30 per cent of hydrogen, I think you will find about 33 or 34, I can not see how you can put natural gas into the blast furnace, or into any furnace, to deoxidize metals, that will have 30 per cent. of hydrogen. The resulting gas of that will be water, an oxidizing gas. Now, I have gone all through that. I have taken out some patents. If you will look in 1866 or 1867 you will find about 40 claims on the use of hydro-carbon vapor, this same natural gas.

Experiments in this direction have cost me $10,000, to dephosphorize with natural gas and I did dephosphorize sometimes and sometimes I did not.

MR. JARBOE—In regard to Mr. Beal's furnace, in the first place if this marsh gas is passed through regenerators, the gas is decomposed, the carbon is deposited on the chequer bricks, and the hydrogen is set free and takes the shortest and quickest road to the chimney before its heating properties are extracted.

Secondly, this gas requires much more air than carbonic oxide, and the air chequer in regenerative furnaces is not large enough. The proof of this was seen at Carnegie's mill, there they tried to use the gas admitting air only through one set of chambers—the air regenerators—it was not successful, but after closing the valve from the producer, and with an outside opening letting the air pass through the gas cheques as well as the original air regenerators good results were obtained.

MR. JONES—I have a few ideas I gather from the report. I think it was a mistake—you will pardon me for making a suggestion—I think they should have embodied in the report a few instructions how to use the gas. Now, for instance, I noticed in the papers the other day at an establishment on the South Side where they are using the gas the foreman of the works turned the gas into the furnace then lit it then lit out himself. It would be well to put into the report to start the fire before you put the gas on.

In regard to the question as to the commercial value of the gas, if Mr. Metcalf has been bothered with 82 firemen and coal heavers, as I have been, one-half belonging to the Amalgamated Association and the other half to the Knights of Labor, he would be glad to get the gas at any price. Whenever I went to that boiler house I felt as if I was going inside the walls of a penitentiary and every one looked at me as if to say "Have you got permission to come here?"

And in this connection I would say and I say it with pleasure that there is very little difficulty in using it. I think probably the greatest difficulty in using it is to get sufficient air to have perfect combustion. We have two reverberatory furnaces that were built especially not to overheat the steel but it was found impossible to get good heat on the steel. When we came to use the natural gas we found the same difficulties through only the ordinary draft by grate bars. We did not get the proper results. We turned on the blower and blew the air in, and now we have no further trouble there.

For the benefit of the concerns in Pittsburg that may use the natural

gas I can lay down as a rule, see that you get plenty of air. That seems to be the greatest difficulty.

MR. DURFEE—I would inquire if this gas could be used as a motive power in the cylinders of engines, and then turned into a reservoir or storage? By such an arrangement as that the greatest possible economy could be obtained from the use of gas, first using the gas as a motive power and second as a store-house of heat.

MR. JONES—The question of the gentleman as to using the gas first as a motive power and then utilizing it by storing it for fuel is proper discussion for this Society. For instance, we have an 8″ pipe at the Edgar Thompson Steel Works through which we get the gas for making the steam for 4,000 horse power every 24 hours, and do heating for 600 tons of rails. In other words the 8″ pipe wil represent somewheres about 400 tons of coal every 24 hours. That 8″ pipe will only drive one of our ordinary engines, while it will supply enough gas, at 65 pounds pressure, equivalent to 800 tons of coal, so that the question of using gas as a motive power is out of the question. The most economical way is to use it in generating steam.

MR. PAINTER—I came here for facts. For the purpose we have to get something cheap. I may not be a fluent speaker but I am going for facts. I have seen every gas furnace in the city, Carnegie Bros. & Co., Wilson, Walker & Co., Spang, Chalfant & Co., and they are all expensive with one exception. There is one furnace I will except, that of Spang, Chalfant & Co's. I can take good bricklayers and change about four a day from a coal furnace to a gas furnace at a cost I suppose not to exceed $5. This is what we want. They use their gas very economically at a pressure of about 2½ pounds. Carnegie Bros. & Co., have a furnace there, a reversing furnace, which cost in the neighborhood of $470 per furnace.

MR. JONES—Siemens furnaces require very little. I have not a particle of doubt Mr. Painter will find an opportunity of using regenerators for same cheap. I have seen them at Wilson, Walker & Co'., with slight modifications, modified from the Siemens furnace. You must get air as much as possible.

MR. ROBERTS—I would call Mr. Jones' attention to the table which shows the explosive results of different mixtures of natural gas with volumes of air. He might get some information from it.

MR. METCALF—I think it would be well for Mr. Painter to consider another matter; a change from one system to another means very little from first cost providing you get corresponding results in the end. I know of a very small concern that spent $75,000 in throwing away old furnaces and putting in new furnaces and got it back every year. If Mr. Painter can get $4 or $5 furnaces and they give the same results then he has got a good thing and he can afford to give it away.

But Mr. Jones misunderstood our friend in the rear in regard to using gas as a motive power. The gentleman suggested the use of gas in steam cylinders and using the waste gas for heating. He was right, because it certainly is not sensible to burn this gas under enormous pressure.

MR. JONES—You misunderstood me. What I wanted to say was this, with that 8-inch pipe, which is saving us at least 400 tons coal per day, we could not run a rolling mill train. That 8-inch pipe would be equivalent

to 800 tons of coal, I think. The economy would be to use steam in engines and generate steam by the natural gas in preference to using gas as a motive power.

Mr. Henneck—With the pressure you would have you would not require another engine back of that gas to bring your gas there, if you use the pressure to do other work. You must have 6 inch water pressure of gas in order to use it at the furnace, at least. If you take the pressure off, getting the work required of the machine with the same driving power back of it, and get the gas there, in that way you can utilize the pressure in the gas beside using the gas as fuel.

Mr. Jones—We use an 8-inch pipe in order to supply the gas to works and we find it takes a 10-inch pipe to run the rail mill engine. If you take an 8-inch pipe with 65 pounds pressure, then you will have to take a reservoir and store your gas up into a tank. It is hardly a problem to be considered for if it takes a 10-inch pipe, with 70 pounds pressure, it is hardly fair to suppose an 8-inch gas pipe, with 65 pounds pressure, will do the work, as Mr. Metcalf suggests.

Mr. Metcalf—Our friend is barking up the wrong tree again. I did not recommend the system at all. I merely stated what the gentleman had said.

Mr. Jones—It is a generally settled fact that it is possible, and I think it is as good a way as any, when using the gas under pressure, to use the injecting principle to get the air in. But for heating purposes, furnaces especially, it is a pretty well settled conclusion that it is necessary to get down to using regulators and low pressure. However, members will all have full opportunity of seeing the gas working in our shops, etc., for the mechanical genius of Allegheny county will soon have it tested in every shape and form to see what is the best form. Mr. Park, I believe, has an ordinary draft furnace, such as generally used in Pittsburg, and he uses a blower. You will find a good many blowers around Pittsburg.

Mr. Jarboe—About this injecting principle—I have an analysis of chimney gases: of 24 or 26, (I think,) different boilers being used with injectors, and have an analysis of several used with a blower to put the air in. The analyses show less waste by a large majority with a blower putting the air in, and there is also a great deal less gas burned and a great deal more steam made by same style of boilers. It is a very difficult thing to get an injector that will work under a variation of a few inches. I have made a number of experiments with different injectors and different blowers, and in no case have I found an injector that will do satisfactory work. I have a little furnace, a little bit of a baby, that is burning with flameless combustion by using gas with blast. That little furnace will melt down Benezet brick, and it is heating rivets. Major Munroe will tell you there that very few of those rivets have been oxydized. My idea of a furnace is to put a blast on it, when you can get it. You have perfect control of your air. You have perfect control of your gas valves and you can mix the two just as you please.

Mr. Jones—What pressure have you made it?

Mr. Jarboe—I have tested it by inches up to 20 inches, then by ounces up to two pounds and then by pounds up to 175 pounds pressure, every pound from two pounds up. Remember, gentlemen; I used a gas holder.

I metered the gas that went in and then I metered the resulting gas in the gas holder so I should know exactly what proportion I had and I never found an injector yet that mixed it right.

Mr. Reese—While I agree that the greatest difficulty to be encountered is in getting enough fresh air to mix with the gas in use, I only want to say one thing. In conversation with some stockholders of the Citizens Passenger Railway the other day, it was stated that some parties had struck a very large gas well not far away and they were considering the feasibility of bringing it in to drive their road by cable, that is to drive the machinery by which the cable would be operated by the gas and selling their excess for heating and light.

By a member—I think this matter can be stated in this way, that the gas following through these mains has two kinds of properties, one dynamic and one chemical. Heretofore it has only been used in its chemical capacity. Certainly if a flow through an 8-inch pipe will give that pressure of 65 pounds to the inch most of us, who don't live in Pittsburg would think that it can be used dynamically without impairing its efficiency. It would seem to be worth doing. That is a point of suggestion made at the other part of the room.

Mr. Jarboe—I will answer this point. Sometimes this gas we talk so much about is a minus quantity. Shoenberger & Co., and Hussey, Howe & Co. had to shut down to-day on account of not having gas enough, so it is a minus quantity. At the wells the pressure rises up very high. You take a well flowing so that it will run up to 175 pounds to the square inch in a minute and that will be reduced down to 105 pounds by friction in the pipe, and by the time you get it way down town it is not worth much. It is used in the oil country largely where they put it into the engine, but the gas brings up with it so much sand that it is very hard on the engines.

Mr. Roberts—The question before the public in Pittsburg turns a good deal upon the domestic consumption of natural gas and the various means looking to the safety in use of this new fuel. It has been in use for a number of years, as stated in the report, in smaller villages, but when it comes to introducing it over a large city like Pittsburg it is a different matter. People do not know all the conditions of pressure, etc., etc., and it will be of importance to the companies to post consumers as to the way to handle it in such a community as this, and it is looking toward that point, that we illustrated here so particularly the forms of tank governors to hold this pressure down to the minimum of safety. It was suggested during our examination, that these are questions the public take more interest in than its utility for manufacturing purposes.

I will state for the benefit of our friends that here are specimens of the various forms of joints referred to in the report.

Mr. Jarboe—(Showing the various styles of joints). Here is a light casing or light wrought iron pipe, and here is heavy pipe, 28 pounds to to the foot, 8-inch pipe. Here is 6-inch heavy and here 6-inch light casng with taper joint of the National Tube Works. Here is one with taper thread and taper socket to match; here is an ordinary straight socket joint, a Converse lock joint; that light pipe is put together with the lead being

poured in here, just the same as cast iron pipe. That is the one that has been used in Oil City.

Here is a little joint that is made with ordinary standard pipe and standard thread. The ends of the pipe are beveled. One end has a lead rim put in and the other end of the pipe is put in here and screwed together. They force the lead into the thread and into the thread of the socket and also between the two ends of the pipe, and it makes it a very good joint Here is a taper socket. The socket is tapped from both ends and the pipe is cut to suit. I do not know that every person here is aware that the pipe sockets are tapped with a straight tap right straight through. The thread of the tap is cut at right angles to the face and therefore all the threads are at right angles to the face of the socket.

These samples here are used by the Fuel Gas Co., (indicating certain ones). The others are ordinary samples.

Mr. Durfee—On this question of pipe joints it is pertinent, I think to say that a joint somewhat similar to those which have been described, in the use of a lead ring, has been found very satisfactory in the joints connected with the Emery Testing Machines. But to prevent the flowing of the lead in the bore of the pipe and its escape in that way, and also the formation of a ridge in the interior of the pipe, one of the abutting surfaces is grooved and the other is left either flat or with a corresponding ridge. On every surface the lead is reduced to a very small amount, but it is fully otherwise as perfectly and absolutely as tight a joint as can be found. We have used that joint under very high pressure, 800 or 900 pounds to the inch, and we have no reason to doubt it can be made tight under any pressure.

Mr. Jarboe—Did you ever make a pipe joint like that and take it apart to see if any lead was forced into the pipe?

Mr. Durfee.—We have made some like those I described.

Mr. Kent.—One of the most important questions has not been fully touched on. What is the public value of the gas. Mr. Jarboe gives some figures to-night in regard to the value of this gas as fuel, in answer to the questions raised by Mr. Metcalf. If I understood him aright he stated that the gas was worth $7\frac{8}{10}$ cents per 1,000 cubic feet. In order then to make fuel gas economical to the manufacturers to use it by the wholesale they must reduce the price to $7\frac{8}{10}$ cents per 1,000 cubic feet. I do not think this will be practical, and if I may venture an opinion, I will state that I do not believe natural gas will ever be used on a wholesale scale by the manufacturers of Pittsburgh. But there is a larger field in using it for domestic purposes and it will be used even at a cost of so much as 50 cents per thousand feet. I look forward to its development for this purpose, and not for manufacturing purposes, and the sooner the gas companies realize this the better it will be for them. They must realize that the gas must be sold by meter. They will find that the supply is limited and that it will be far better to look towards its use for domestic consumption and not for wholesale consumption.

Mr. Roberts—Mr. Kent thinks exactly right. I want to give notice that if any of our friends desire to see the gas used in the best form and by a number of ingenious contrivances in a private house, they will find it at the residence of Mr. Howard Morton, in the Twenty-second Ward.

Pittsburgh. It is a nice carriage drive all the way and after getting there they will have a fine view overlooking friend Jones' place, the Monongahela River and the most superb view in the City of Pittsburg. I think it would repay those who desire to visit the place. Mr. Morton requested me to mention this to the Society. He will pilot any gentleman who will go up.

Mr. Jones—This fine view he speaks of is looking away from Pittsburgh.

Mr. Kent—One of the things that people have been looking forward to is to diminish the smoke of Pittsburgh. My opinion is that the manufacturers will not use the natural gas, and that Pittsburgh will still be enveloped. But inside of 20 years Pittsburgh will be a comparatively clean city. It will be a clean city from the introduction of natural gas for domestic purposes, which will stop the flow of smoke from the chimneys of the houses. These three elements are what make smoke to-day—private houses, steam boilers and iron furnaces—and smoke can be prevented in all of them. I will repeat the prediction that Pittsburgh will yet be a comparatively smokeless city.

Mr. Webb—I would ask first, what is the pressure of the gas when the price is $7\frac{8}{10}$ cents per 1,000 cubic feet. The second point is whether this gas has been used in gas engines, and if so whether the natural pressure was sufficient to do the work required, and third, could not the question of pressure be regulated by using larger pipe?

Mr. Jarboe—Figures have been taken at 5 inches pressure and then reduced to a still lower pressure and it is found that the gas would still be worth $7\frac{8}{10}$ cents. This question of large or small pipe has been figured down very fine, and in consequence the Fuel Gas Company are laying two 8″ pipes where before they laid down casing. The gas engine question is answered in this way. The American makers of this engine will not furnish an engine to burn the gas, but the English makers say, send on your orders and we will send you engines.

Mr. Roberts—Mr. Morton, whom I referred to, is here alongside of me, and he has some information which might be interesting to the Society in regard to the cost of gas compared with coal for private use. If the Society would like to hear from him he will speak.

Mr. Miller—This gas is used in another form by the Carbon Black Company in making ink for printing purposes.

Mr. Painter—I have figured this question about the cost very thoroughly and I think in making iron, natural gas is not worth over $1.50 per ton to the finished ton of iron. That is all it is worth, and perhaps less.

Mr. Morton—In regard to the cost of this gas for domestic purposes. For the laborer who cannot indulge in luxuries, he could not afford to use it if it cost more than coal. We have used it at our home in the Twenty-second Ward since the first of December, and when you take into consideration the saving on carpets, wall paper, furniture, labor, and the magnificent satisfaction it gives in every respect, you don't stop to consider the difference between its cost and that of coal. We burn it there in range, furnace and grates. We get the very highest results from it, and the satisfaction is perfect in every respect. There is no fault to be found

with it; it is so clean, so prompt. Breakfast can be cooked by it in a wonderfully short time, and the gas is always ready. No stuffing of chimneys in summer. Often you want just a little fire when it is too warm to burn coal and not warm enough to do without fire at all, and then this gas comes in nicely. We would not be without it now.

MR. DEMPSTER—I would ask the committee if they have made any attempts to odorize this gas for domestic purposes? How are you to know where there is one part of gas to certain volumes of air? Have you made any experiments relative to odorizing the gas by which it can be detected when it is escaping?

MR. ROBERTS—Experiments in odorizing this gas have been very unsatisfactory so far. It has been tried in a number of instances. It was suggested in one of the morning papers that it be passed through the Pennsylvania Legislature.

MR. KIERSCHOFF—I would like to ask if the committee has any record of any actual explosions.

MR. JARBOE—Yes, sir. I was blown about 20 feet once with it. I had my clothes torn, and as fine a boiler setting blown down, as you ever saw built.

MR. KIERSCHOFF—Mining engineers are rather afraid of this marsh gas, and you would make their hair stand on end if you propose bringing it into their houses. This is the most dangerous gas of all.

MR. JARBOE—We have tested this gas in several different connections. My explosion was due to pure carelessness, although I have been burned several times. My man turned on the gas and then lit it, whereas a fire should be built first and then the gas turned on.

As to odorizing it, if you will use a very small quantity of bisulphide of carbon, so arranged that it can be blown into the pipes when the gas is moving through the pipes, it will produce an odor as loud as ordinary carbon oil. But you must use a very small quantity, as any large quantity will make it very dangerous, and therefore it has been thrown aside as a kind of dangerous experiment to try.

WILL A DROWNED PERSON BE RAISED BY THE DISCHARGE OF A CANNON?

BY JOSE DE CUENTO.

[A paper read before the Engineers' Society of Western Pennsylvania, September 16, 1884.]

From the title of this paper it would appear that a solution has been found for a belief, which the writer claims to be a mere superstition, as many others are, but such is not the case.

The writer will simply endeavor to show that there is no ground for such a belief either in the evidences of the scientific or practical world. No scientific writer or thinker has at any time, either past or present, put forth a theory in which it was said that by artificial means the laws of gravity can be destroyed, and yet in claiming that the body of a drowned person will be raised from the bed of the river it will be as much as to say that gravity can be destroyed by a mere shock. That there are many questions which the scientific world ought to destroy at once there is not the least doubt in the mind of the writer, but that the scientific world is perhaps busy with great things and may scorn and look with contempt on small ones there is no doubt; and yet it has been with these that great objects have been accomplished, such, for instance, as the discovery of electricity, telegraphy, electric light, steam, and many other subjects of vast benefit to the world at large. Who could have suggested to the first person to notice that such a phenomena as electricity existed in the earth; that so vast a revolution would be accomplished; that the world would put itself in communication within a very short space of time; that events taking place between two continents thousands of miles apart would be communicated to one another in a very few moments; that the streets of cities would be illuminated with a light almost equal to that of the sun, and many other things which are accomplished by electricity; or to the discoverer of steam, little did he dream when he first took notice of the cover of his kettle being thrown off by a power unknown to him, and that through his endeavors the power of steam was discovered, that the world would have to be put on a different basis, that the works of man would have to be accomplished so as to suit that little phenomenon of nature

that the oceans would be crossed in a very few days and that distance on land would be overcome by the use of this little discovery of his.

Again one of the greatest of modern philosophers, Sir Isaac Newton, whose greatness was not due to his investigation of great things only but to his continuous notice of little ones. His greatest discovery was not obtained by taking notice of the worlds around us but by taking notice of the little things that were taking place around him; by the fall of an apple from the tree the laws of gravity were developed.

It is not claimed by the writer that any great thing may be accomplished by this paper, nor does he claim that to oppose such a belief is doing any great thing, for the writer is well aware of his incapacity, but he does hope that it may be the means of overthrowing or at least of opening a road whereby another with greater abilities may take the subject into hand and completely destroy a belief which is beneficial neither to science nor to the world at large.

This subject is one in which the writer has not the benefit of any theory from the opposition but simply the assertion "I have seen it," but no reasons are given, and when any such are given they do not stand scientific dissection.

The points to be considered in this paper with respect to the belief that a drowned person will be raised from the bottom of a river are, first, sound, or the effects produced by sound; second, the bursting of the gall bladder or veins; and lastly, what the writer claims to be the proper way to find the body of the drowned when the depth of water allows it.

The belief that the report of a cannon would bring the body of a person to the surface was never brought to my notice until the 8th of June last. Although I have heard of cases of drowning from childhood, and even where expense to recover the body of the lost one would not be spared, I never heard that application was made to the cannon but to the dredge; and this the writer claims to be the most efficacious way of recovering the body. On that day a boy was drowned at the foot of Anderson street, Allegheny. I noticed that a cannon was brought to the place where this unfortunate affair took place and discharged several times with the hope or belief that it would bring the body to the surface, but no such result took place. Some inquiries were made as to the motive of using that cannon at that place; it brought the reply that they were looking for the body of a boy drowned there. With this much data the investigation began.

It is claimed by the advocates of this theory that the shock of the cannon against the atmosphere will so affect the water that it will bring up the body. It may be well to find first what is the cause of this shock. We know that whenever a sudden shock is produced sound will be the result, and we can tell by sound produced in the air how far a shock will go. But before going any farther let us see what natural philosophy says with reference to sound. "Sound produced in the atmosphere has but little effect in water, and sound produced in water is very feebly heard in the air, and in a position somewhat oblique is not heard at all." It is claimed that the shock will set the objects at the bed of the river in motion, and particularly so the human body, as it has some buoyancy. Let us see through what mediums must sound pass so as to reach the

body. The sound or shock must be created in the atmosphere and this medium is the poorest one to produce sound that it may be imparted to another one of greater density. We are all aware that a locomotive may be heard when miles away by applying our ear to the rail, the particles of the rail are closer to each other than they are in the air, thus carrying sound farther away than any other medium known; the earth, on account of its density, will be the next medium by which sound will be heard, and lastly, by the air. In the present example the original medium is the rail, and sound in iron travels about seventeen times faster than in air. The earth is a good conductor of sound, but sound in the atmosphere travels with a velocity of 1,090 feet per second. The case under consideration is the reverse of this. Sound, or shock if you please, is created in the atmosphere from this. It must be communicated to the earth and water, both of which are denser than air, but sound produced in air has no effect in water. I cannot conceive, under this condition of things under what principles of natural philosophy this theory is advanced. But granting that the shock will reach the body, is there any reason why the effect will raise the body? What has caused the body to sink? Is there any cause to make the human body sink? Evidently there is. Gravity is at all times and everywhere. If such were the case that the shock would reach the body in the water, the only effect produced upon it would be to sway it to and fro. No one has seen an object lying on the surface of the water that as the waves passed it caused the body to sink or raise itself any more than while the wave passed, but the body kept always at the same depth. Again, as to the idea that the shock will disturb the bed of the river, I have seen cannons discharged over crystalline rivulets, and not rivers, and no disturbance took place, yet sometimes when a thunder storm is passing over some marshy place bubbles of air will be seen to rise from the mud, but this is not caused by any power of attraction that a thunder clap may have, but the shock produced by it. A thunder clap being so powerful and of so large an area the vibrations of the earth produced by it are far greater than anything that can be produced by human power; yet these bubbles are not air but marsh gas whose specific gravity is less than water, and not so with the body.

Again, the shock has to pass through three mediums, one the air where it was originated, the others the water and earth. Sound, as it is well known, has the same properties that light has in coming in contact with a denser body. It is reflected back without producing any great effect, and the greater the elasticity of the body fired against the greater will be the loss of power of the shock.

There is another common belief, and that is, the shock will produce a vacuum over the bed of the river and thus force the air from the water. It is not very difficult to see that this is not true, for if such were the case not only the air will rise in a body, but it will also bring the water with it; for the air and water are so mixed that one cannot be acted upon without affecting the other; granting that such would be the result, that the shock will extract the air from the water, the water in that vicinity will be raised in a ridge, for the water around that vicinity will be forced in that direction by atmospheric pressure, will become denser and the body will have a greater pressure upon itself, and if there were any air in the body before

the firing of the cannon it would be separated and thus, in place of making the body lighter, it will become heavier and the contrary of what was sought will take place. Again, if there had been any such thing as a vacuum created, let us suppose an 18″ gun was used. The area covered by the shock will be proportional to that of the gun. Suppose that at 20′ from the gun the area of the shock is eight times that of the gun, a very insignificant thing compared with the area of the river, and the air be extracted from this track, there are 14.9 lbs. pressure per square inch of air, and this pressure, at the moment it finds a point where the normal pressure has been diminished, will force the water to that point, thus raising a column of water on the path of the sound, which will increase the depth of the water, and at the bed its weight, again producing opposite results from what was intended.

I have seen the air extracted from water under the receiver of an air pump, but it was obtained with difficulty. Thus, if it was difficult to extract the air under a vacuum how much more difficult will it be in the open atmosphere. Moreover, what will be the effect produced by the expansion of gases? On their reaching at the nozzle of the gun they will drive the particles of air in every direction. The results will be as follow: When the gases are ignited they combine chemically and their union is accompanied by intense heat. The air at this hot focus expands suddenly, forcing the surrounding air violently away on all sides. This motion of the air close to the cannon is rapidly imparted to that further off, the air first set in motion coming at the same time to rest. The air at a little distance passes its motion on to the air at a greater distance, and comes also in its turn to rest. Thus each shell of air, if I may use the term, surrounding the cannon takes up the motion of the shell next preceding, and transmits it to the next succeeding shell, the motion being thus propagated as a pulse or wave through the air, but those that reach the water are repelled, water being of greater density and elasticity, and the greater these two are the greater will be the repulsive power and no effect whatever will be produced. Professor Tyndall, in his lectures on sound, says "the velocity of sound in air depends on the elasticity of the air in relation to its density. The greater the elasticity the swifter is the propagation; the greater the density the slower is the propagation. The velocity is directly proportional to the square root of the elasticity; it is inversely proportional to the square root of density. Hence, if elasticity and density vary in the same proportion the one will neutralize the other as regards the velocity of sound."

Much more could be said of sound, but I believe this to be sufficient to show that under no case will sound or its results bring anything from the bed of rivers.

With reference to the bursting of the gall bladder or the veins of the body, I do not believe that the advocates of this theory could have invented a more ridiculous principle by which to deceive themselves and others as well. Physiology says very plainly that the arteries and veins are formed of a muscular tissue. Surgery also states that the most difficult thing to cut is muscular tissue. The veins being hollow, and besides their power of expansion and contraction when any pressure is brought upon them they can reduce themselves to a smaller space, but to burst a vein or

artery in any portion of the body, such portion would have to be reduced to a jelly or else the pressure must come on the inside of the vein, two things that cannot be accomplished in the water. Although it may be granted that a shock will cause a heavy pressure to come upon the body, the pressure will come on the whole of the body and not on one particular place. Nature has so constructed the human body that the most sensitive parts, and those upon which life depends mostly, are so protected in a body that nothing can affect them except when cut or any member of the body has become mangled. I have heard of instances of the blood issuing from the ears of artillerymen who had stood for a long time at their guns during heavy cannonading. Even under this condition the blood did not come from the veins or arteries, but from the capillary tubes; but these are different in structure from arteries and veins. The shock had to pass but one medium, and since sound acts inversely with the square of the distance it is pretty safe to say that an artilleryman standing not more than 3 feet from his gun, the action of the shock on the tympanum of the ear is so powerful that it may break all the nerves in the ear. Anyone who has ever cut himself will notice that the blood will not spring from the wound unless a vein has been cut, but will flow out as though it were water coming out of a filter. Medical men will testify to this fact, such as have practiced during the age of blood-letting, that it requires not only a sharp lancet, but also a steady nerve and considerable skill to cut an artery or vein. Physicians will say that a person can drive a knife through any portion of the body, and it may come in contact with a vein or artery, but if the vein or artery is not struck in the proper place the knife will glide to one side and pass on; it may kill the person but will not in the least affect the correctness of this statement. But we will suppose that the veins will be bursted. Is there anything gained by it? There will be nothing gained by it; the blood will remain in the body coagulated. The specific gravity of the blood in its normal state is 1055, that of water 1000 and it is found that after it has become hard the specific gravity increases. Moreover the human blood is found to coagulate about 12 or 15 minutes after it leaves its normal position, but if the blood is allowed to remain in the arteries and veins liquid blood will be found in the region of the heart and lungs. "It has been found 24 hours after death." It will be seen that by bursting the veins nothing will be gained. Although gravity is increased but little, it will be something toward increasing the specific gravity of the body and not to make the body lighter.

The above are the crude ideas that the writer has with reference to raising the body from the beds of rivers. They require the master hand to beautify them, yet it is the hope of the writer that they may be the means of sparing unnecessary pains and expense to those who may have the unhappiness of seeking some dear one lying at the bed of a river.

It has been shown, I believe, that sound or its effects have no effect on the body under water, and that the bursting of the gall bladder and veins have a tendency to increase the weight of the body.

The following, taken from Marsh in his Outline of Philosophy ought to settle all points without any doubt: "The specific gravity of the body depends upon that of its various tissues and organs. Essentially, all the materials of the body, with the exception of the fatty substance, are

heavier than water, and the main specific gravity of all the tissues is higher than that of water. But the air retained in lungs during life, even the residual and reserve air, is just sufficient to counterbalance the higher specific gravity of the body generally, and so enables it to float.

The specific gravity of the entire body with air in the lungs is usually stated to be from 1060 to 1070. As bone is the heavier and fat the lighter of the tissues, the specific gravity of the entire body is influenced by the relative proportion of these two tissues, hence it is greater in bony persons, but less than the average in children and women, who are generally fatter than men, and also in corpulent persons of both sexes. But the practical buoyancy of the body in water, is, of course, chiefly determined by the size of the chest and lungs, the freedom of the latter from congestion or deposits, and their condition of inflation. On the least inspiratory or expiratory movement the body rises or sinks in water. Necessarily the body is lighter in sea water than in fresh water.

The effect of wet cloth has a tendency to increase the weight of the body. (Marsh, Outline of Philosophy, pp. 195, 196).

Finally it will be seen that no artificial means except the dredge or diver can bring the body to the surface of the water. Nature must take its course, and the body will rise in due time. There are certain natural laws that man should not attempt to change as they are established by nature and they must stand. One of these is gravity. Gravity is one of the laws that puzzled philosophers for centuries; how the worlds remained in their position and what held them. This was left for Sir Isaac Newton to settle; Nicomedes to find that there must have been something that caused a body to sink. Can it be possible that a new law has been found at this late date by means of which gravity can be overcome? Then there must be some means whereby we can overthrow that force that holds the solar system together. But it will be a waste of time to think of a subject that will tend to advocate that by a shock gravity will be overcome. Therefore in cases of drowning the best way is to dredge for the body, and if by this means it cannot be found then let time do its work. Putrefaction will take place and this will be influenced both by the state of the person and food that has been in the stomach before drowning. There are persons whose systems are not in a good state, and if such were drowned in a place where the temperature is above normal, decomposition will set in sooner, and as soon as the body has become filled with gases it will rise. Decomposition will be controlled by the state of the body, food, and temperature of water.

DISCUSSION.

Mr. Brown said: The paper exhibits a great deal of thought and preparation. And yet there is one theory mentioned that I do not quite agree with. I have seen gravity overcome very frequently. You take some of these restaurants that do not make coffee very well, you stir it up with a spoon and the sediment stands on the top of the cup, whereas by the natural law it should go to the bottom. Now, if we fire a cannon, we simply disturb the atmosphere, which in its turn disturbs the water and forms a series of undulatory inclined planes, which in turn, move or work

on the body, and so the body is worked or moved up this series of inclined planes. I would state that I would not care to have my own body used to experiment on in this manner.

MR. DEMPSTER: The paper is very good, but as the hour is late and I did not hear it well, I move that the discussion be postponed until the next meeting, so we can, in the meantime, read it and be able to discuss it intelligently.

MR. ROBERTS: Before we take a vote on that motion I would like to state that, happening to be in Allegheny City that day, I saw the experiment mentioned. The only effect I saw was that they blew in the side of a coal barge and it cost them about $500 damages.

A SPECIMEN OF CAST IRON PIPE.

BY JAMES H. HARLOW.

[Exhibited before the Engineers' Society of Western Pennsylvania, October 21, 1884.]

My attention was first directed to the sample of pipe before you by my friend, Mr. H. C. Dickinson, member of this Society. The sample shown is from Fall River, Mass., and was laid in 1875, and at the time it was taken up had been in use eight years.

HISTORY.

The water works of Fall River, Mass., were built in 1871–5, and take water from Watuppa Lake. This lake covers a surface of about 5½ square miles.

Professor J. H. Appleton in his report on the water of this lake says: "That a careful analysis of a sample of the water from North Watuppa Pond showed but 1.80 grains of solid matter per gallon, and that the purity of the water, its freedom from objectionable salts, and remarkable softness, render it eminently suitable for the various purposes of a water supply."

The chief engineer of the work was George A. Briggs, with William Rotch as assistant engineer, and James P. Kirkwood as consulting engineer. The sample exhibited was a part of an order filled by the Warren Foundry and Machine Company, Phillipsburg, N. J., and consisted of about 800 pieces of 6-inch Class D pipe, ⅝-inch thick, having an average weight of 514 pounds each.

The following extracts from the specifications are given to show the quality of pipe the engineer intended to have:

QUALITY OF METAL.

The metal shall be strong, tough and close grained, with the carbon combined and not in the form of graphite, and as hard as the case will admit, but not too hard to be readily cut and drilled, and shall be remelted from pigs of grey iron in a cupola or air furnace, without any admixture of cinder iron or other inferior metal, and shall have a tensile strength of at least 16,000 pounds per square inch.

DEFECTS AND FORMS OF CASTINGS.

The pipes shall be free from scoria, sand-holes, air bubbles, cold short cracks and all other defects or imperfections; they shall be truly cylindrical in the bore, straight in the axis of the straight pipes, and true to the required curvature or form in the axis of the other pipes; they shall be internally of the full specified diameter and shall have their inner and outer surfaces concentric, so as to make the thickness of the shell uniform throughout.

The castings must be smooth and true, and shall be perfectly cleaned from earth, sand, or dust which adheres to the iron in the moulds. No lumps or rough places shall be left on the inside of the pipes or sockets, nor on the outer surface of the spigot ends. No plugging or filling will be allowed.

DIRECTIONS FOR CASTING.

The straight pipes shall be cast vertically in dry sand moulds with the end down, and the curved pipes and branches may be cast in loam. The flasks of all pipes shall be allowed to remain in position a sufficient length of time to prevent unequal contraction.

SCHEDULE OF PIPE.

Class.	Nominal Diameter.	Exterior Diameter.	Thickness of Metal.	Standard Weight.	Gross Weight.
	Inches.	Inches.	Inches.	Pounds.	Pounds.
A.	12	13 7-8	1-2	822	
B.	12	13 7-8	5-8	1018	
C.	12	13 7-8	25-32	1255	
D.	12	13 7-8	15-16	1475	
A.	10	11 5-8	15-32	642	
B.	10	11 5-8	19-32	802	
C.	10	11 5-8	11-16	889	
D.	10	11 5-8	13-16	1041	
A.	8	9 1-2	7-16	489	
B.	8	9 1-2	17-32	581	
C.	8	9 1-2	5-8	684	
D.	8	9 1-2	3-4	812	
A.	6	7 1-4	13-32	341	
B.	6	7 1-4	15-32	392	
C.	6	7 1-4	17-32	439	
D.	6	7 1-4	5-8	514	
A.	4	5 1-16	3-8	215	
B.	4	5 1-16	13-32	233	
C.	4	5 1-16	15-32	265	
D.	4	5 1-16	17-32	298	

No pipes will be received which fall more than 4 per cent. below the specific weight for the class and size of pipes to which they belong, and no excess of more than 4 per cent. above the specific weight will be paid for.

SPECIMEN BARS.

Specimen bars of a suitable size for testing, shall be made from the metal of each heat, if required by the engineer or his duly authorized inspector. These specimen bars shall be poured from the ladle at any time, either before or after the pipe has been poured, as may be required, and shall present a true specimen of the iron used for making the pipes. Such specimens when so made, will be received and labeled by the inspector, who shall take the necessary steps, if required by the engineer, to ascertain their tensile strength.

REJECTION.

The engineer or inspector may reject, without proving, any casting which is not in conformity with the specifications or the plans furnished.

DELIVERY AND INSPECTION.

All pipes and special castings embraced in this contract must be delivered sound and in all respects conformable to the specifications. The inspection will not relieve the contractor from any obligation in this respect, and a defective pipe or casting, which may have passed the inspector at the works, or elsewhere, will be at all times liable to rejection, when discovered, until the fulfillment and adjustment of this contract. The payments herein stipulated to be made are predicated upon the receipt of sound and perfect castings.

All of the pipes and special castings must be thoroughly cleaned and freed from all earth, sand or dust, and no acid or other liquid shall be used for this purpose. In finishing the process, hard brushes must be used to remove any rust or loose material which may be upon the castings, and after a thorough and careful hammer inspection,

to the satisfaction of the engineer or inspector and according to the specifications, the coating shall be applied in the manner and under the condition following:

COATING WITH COAL TAR VARNISH.

Every pipe and special casting must be entirely free from dust, sand, or rust, when the varnish is applied.

The varnish or pitch is to be made from coal car distilled until all the naptha is removed, the material deodorized, and the pitch reduced to about the consistency of wax or very thick molasses; pitch which becomes brittle when cold shall not be used.

The pitch must be heated in a suitable vessel to a temperature of 300° Fahr., and must be maintained at that temperature during the dipping, and fresh pitch must be frequently added, with at least 6 per cent. of heavy linseed oil, and the vessel shall be entirely emptied of the pitch and refilled with fresh material as often as may be necessary to insure the perfection of the coating.

Each casting shall be kept immersed until it attains the temperature of 300° Fahr., or, if required by the engineer or his duly authorized inspector, each casting shall be heated to the above-named temperature in an oven prepared for the purpose, before being dipped, and shall be immersed in the pitch while hot.

After the coating has been completed, the castings shall be placed to drip in a position which will insure an even and uniform thickness of the varnish.

The coating on the pipes and other castings must be tenacious when cold, not brittle or disposed to scale off, and should it appear to the engineer or his authorized inspector that the varnish has not been properly or satisfactorily applied, the pipe or casting shall be thoroughly scraped, cleaned and re-coated.

WATER AND HAMMER TESTS.

All of the pipes and special castings, after having been coated and become cold shall be subjected to a water pressure of 300 pounds per square inch, and while under this pressure, shall be rapped with a hand hammer from end to end to discover any defects which may have been previously overlooked. Should any pipe or special casting, while under the required pressure, show any leak, sweating, or other defect, it will be rejected.

All of the pipes and castings shall be weighed after having passed the required inspection and proof, and the weight shall be conspicuously marked on each with white paint.

RE-INSPECTION.

The Water Commissioners reserve the right to inspect or re-weigh at their own cost and expense, all pipes and castings after their delivery upon the wharf, and any pipe or casting which may be found deficient in weight or defective in metal or form will be rejected and the contractor will be paid only for such pipes or castings as are found to be in conformity with the specifications when delivered.

Class A pipe, mentioned in the specifications, were used under heads of 80 feet or less.

Class B, under heads between 80 and 140 feet.

Class C, under heads between 140 and 200 feet.

Class D, under heads between 200 and 260 feet.

Class D, was, however, put under 275 feet head.

In accordance with a clause in the specifications, the pipes were all weighed on arrival in Fall River, and found correct. This piece was laid on a street along a dock, and is covered twice each day by about 1.5 feet of salt water.

The defective piece was discovered by the pressure bursting the pipe and causing a leak.

On uncovering the pipe it was found that six lengths or 72 feet were in the same condition as the sample. The six lengths were continuous, and at each end the pipe was still in good condition. The material through which these six lengths were laid was rolling-mill cinder, a sample of which is shown. You will notice that the inside coating of the pipe is still bright, except at one place, which was probably the underside, and on which a little sediment has settled, and also shows a spot of rust. The outside coating is still on the pipe, but not in so perfect condition.

The inner part is still cast-iron and appears to be of good quality. The outer line of the iron is very irregular, and at one point reaches nearly to the outside. Along the outside edge of the iron is a streak of red oxide, and beyond this is the silicated oxide of iron.* I make the specific gravity of the sample 4.358. The sample contains 20.45 cubic inches. The weight is 3.25 pounds. The weight should be 5.33 pounds.

A sample has been submitted to Messrs. Hunt & Clapp, chemists, and they have given me the following report:

PITTSBURGH, Pa., Oct. 20, 1884.
Analysis of water pipe. Sample marked "Cuttings Through Pipe." Received from Jas. H. Harlow, Pittsburgh, Pa. Received at Laboratory, Sept. 26, 1884. Remarks: Pipe laid at Fall River, Mass.

Silicious Residue	14.63
Oxides of Iron	76.84
Alumina	7.70
Moisture	0.37
Sulphate of Lime	0.14
Sulphate of Magnesia	0.08
	99.76

Specific gravity, 4.358.

PITTSBURGH, Pa., Oct. 20, 1884.
Analysis of water pipe. Sample marked "Outer Surface of Pipe," Received from Jas. H. Harlow, Pittsburgh, Pa. Received at Laboratory, Sept. 26, 1884. Remarks: Pipe laid at Fall River, Mass.

Silicious Residue	25.18
Oxides of Iron	63.70
Alumina	9.70
Moisture	0.49
Sulphate of Lime	0.19
Sulphate of Magnesia	0.10
	99.36

This has been brought to your notice, in order that we might, by discussion, learn something as to the cause of the change in the iron, and possibly prevent similar accident in the future.

For information given in relation to this sample, I desire to thank Mr. H. C. Dickinson for calling my attention to the subject, Mr. William B. Durfee, Jr. Supt. Fall River Gas Works, for information in relation to its history, and Messrs. Hunt & Clapp for the chemical analysis.

DISCUSSION.

After Mr. Harlow had given the history of the piece of pipe which was handed around for inspection.

MR. HUNT: I can hardly say much more than what the analysis shows other than this, that from the analyses I am quite confident that the outer coating is not the slag that would go with the iron in its casting. It is not either a form of lime, or of the bedding which would have been around the iron, or could have accidentally gotten with it in casting.

*At that point in the sample where the iron extends through to the outer surface the thickness seems to be the same as when originally cast. But at other points the thickness seems to have been increased about 1/16 inch.

Again, all the circumstances of the case, after the careful testing the material had, would seem to indicate as well that this outer coating is something foreign, something not contained with the material when it was made into the pipe.

It seems to me, but I only put this out as a supposition, that the material shows what in mineralogy is called a metamorphosis of the material. The iron is oxydized, made oxide of iron, due to the laying in the cinder bank and being covered with the salt water. And the oxide of iron was afterwards changed, metamorphosed with the clay, forming silicious clay. That the pipe now is of regular form, first of metallic iron, then of the oxide of iron that encrusted it, and is now a metamorphosed material, a changed material from the oxide of iron to silicated clay.

That is only a supposition, and the only remark I wish to make on the subject is to state that that is my idea of the change, and what is the cause of it.

Mr. Phillips: Was any carbon found in that residue at all?

Mr. Hunt: Carbon? We did not test the metallic iron for carbon, but there was none in the material as we took it. The material as we took it for analysis, could be easily whittled—was easily whittled, and could easily be pulverized between the hands, and there was no carbon. The carbon must have been entirely taken out of it. The loss on ignition was only .47 of 1 per cent. The material has a brown color as it looks on the surface of the pipe, and is that way when in powder, but when ignited turns to a black color and looks like the magnetic oxide of iron.

The black portion of the material is magnetic. I think they are small particles of regular metallic iron. One other point, I do not hardly dare to mention, is that this is metallic aluminum. The specific gravity would indicate that there was something in that line. I would rather ask for further information before I would say positively there was metallic aluminum there, but there is certainly about 9 per cent. of the oxide of aluminum, and perhaps some of the oxide is in a metallic state.

Mr. Lowry: Did I understand it to be said that the pipe was buried where salt water, the tide, could reach it?

Mr. Harlow: Yes, sir, about 72 feet of this pipe was so exposed. This entire section was affected like the sample. The rest of the pipe was good, so far as known.

Mr. Lowry: Miles and miles of pipe in Jersey City have been destroyed by the action of salt water. The water was absorbed by the earth. Miles had to be taken up and replaced with other pipe; some where salt water never reached it, but was absorbed by the earth and so carried. It is a known fact that where Government vessels have been sunk, the anchor chains when taken up could be cut with a knife, after laying in the salt water.

Mr. Phillips: It is a common thing for iron to be affected in that way, but it is singular that the carbon should be destroyed. It is known that iron bolts and castings in brewers' vats are frequently turned into a mass of soft graphite.

It seems to me the lightness of the material here is rather difficult to explain unless it be possible it was very porous and the material was somewhat puffed up with air. Could that have been the case, Mr. Hunt, that

the lightness was due to the material being partially puffed up by air rather than to the presence of aluminum.

MR. HUNT: Yes, sir, it might have been. I would not want to state that there was metallic aluminum in the sample. I only put that out as a supposition. The material is certainly lighter in specific gravity than the presence of oxide of iron, from its chemical analysis, would show.

The paper of Mr. Lowe on "Mexican Railroads" was so exhaustive that but little disposition to discuss it was manifested.

MR. FISHER said: I should like to say something, but I was entirely ignorant of this system until to-night, and I would like to hear from some of the others.

MR. STROBEL: I should like to put one question. That is whether there is a large amount of timber in Mexico?

MR. LOWE: In some parts only. Along the coasts and in the interior. That on the coasts is mahogany; in the interior you find pine. There is also common a kind of locust called "mesquito."

MR. STROBEL: What kind of cross-ties do they use?

MR. LOWE: Their cross-ties are the same as ours. They were as long but not as large. They were about 6×8 generally. They used any kind of wood; a great number were of pine. Along the Vera Cruz route they used "———," a very hard wood, that would last from 15 to 20 years.

NATURAL GAS.

BY WILLIAM METCALF.

[A paper read before the Engineers' Society of Western Pennsylvania, Pittsburgh, November 18, 1884.]

Before he began the reading of his paper Mr. Metcalf called attention to a lump of graphite which he had brought with him, and which he stated had been deposited by the gas on the hot walls of a furnace in about three days—a good solid lump of graphite.

The natural gas of Western Pennsylvania, is a product so remarkable that it is worthy of repeated, serious and thoughtful discussion. The gas wells are all situated on a straight line running northeast and southwest, through every one of them. An observer standing on a hill top in Allegheny Township, Westmoreland county, say about three miles southeast of the confluence of the Allegheny and Kiskiminetas rivers, can see on a dark night, on the northwestern horizon, the reflection of the lights from the Butler county wells; to the north, the lights, from wells in the direction of Kittanning; to the northeast, the Leechburgh and Apollo wells; to the southeast, the Murraysville wells, and to the southwest, the lights of the Tarentum wells. Off in Washington County, and down towards Steubenville there are other wells; while at Hulton, in Pittsburgh in the East End, at Soho, at Brownstown, at Sligo, and in Bayardstown, there are wells upon wells, roarers, gushers of salt water and gushers of fresh water, and dry holes, but all contain more or less of gas. These wells are all on the same straight, forty-five degree line. This may puzzle mathematicians and makers of geometries, but we all know that science is nowhere when it butts against facts. It is bad for science but we have the facts. Some of these wells give out their gas at an enormous pressure; a gauge, on a six inch pipe, situated some miles from the well, registered on last Saturday 120 pounds per square inch and the noise of the rushing gas indicated that the gauge was about right. Have we a right to expect a long continuance of these high pressures? is a proper and earnest question, often asked by those who contemplate the expense of changing their plant so that they can use the gas. The able report of our Gas Committee read last May, and the able action of our gas companies, would lead us to answer "no." The report shows that the great roarers only roar for a

few years at most; and the charges of the gas companies show that they know enough to get their investment and their profits while the roaring lasts. Are we then to be without gas in a few years? Most decidedly no. It is a well attested fact that the town of Fredonia, N. Y., has been lighted by the same gas well for more than forty years; and we are told that the Chinese have been using certain gas wells for four thousand years, and that they are still good wells.

But a better comparison is in oil; a few years ago, no oil well was counted that was not a gusher, a mad rage for gushers sent the wild cat-ters all over the country, and gushers were struck without number; there was a great hurrah over each new one, and a scramble for more; our creeks contained mill ponds of oil, our river was hidden under a coat of oil, the fish died of oil, our boilers foamed with oil, our noses abhorred the smell, and our stomachs rebelled against the taste of oil. Wise old men held up their hands in horror at the waste, and knowing ones speculated; the prophets said it would soon all be gone, and the conservatives held on to their cannel coal lands and smiled quietly at the thought of their coming day. But cool heads and strong hands were at work, system and order soon gathered the oil into pipe lines and stopped the waste; the little wells were cared for and pumped regularly, until now we have millions upon millions of barrels safely stored; the pipe line certificates are as good as grain receipts; all civilization has the cheapest and best light and lubricator ever made; gushers scarcely make a ripple in the market, and the oil business is safe, stable, wealth producing and enduring. So it must be with the gas. The few roarers will be replaced by the hundreds upon hundreds of little wells, that will be as permanent as the wells of the Chinese, and the quantity gained will be millions of cubic feet more per day than we dream of now. The craze for roarers has taken the place of the gusher frenzy; men everywhere, on the 45 degree line, are boring; that there is no pipe line near, no mill, no factory, matters not; each drills on full of hope; he puts his hundreds down into a hole, he strikes the true belt, he is on the line, he has a roarer. Up goes his pipe, up goes his flame, up go his thousands, and up goes his hat. This is the lunatic's time, and he must have his day; after that comes the engineer's time, and for that we must be prepared. Leaving out, for the present, chemistry and thermodynamics, heat units, etc., etc., let us look at a few facts that we know. This natural gas cannot be highly superheated, because it dissociates and fills everything with a deposit of hard, fixed carbon. (Exhibits specimen.)

The piece of coke shown here was deposited on the bridge wall of a furnace, in a few days' working. Several pieces of similar coke were piled on a clear, hot fire, and in about an hour they were red hot, but showed no signs of combustion. A blower was then put up before the grate, after fresh coal had been put over the coke, and was left there for half an hour until the coal was all burned off and the fire was white hot. At the end of that time there was no appearance of the coke having been reduced. A farther experiment to-day gave the following results: A piece of coke weighing four pounds and six ounces, was put into the fire at 9 o'clock. It was at once brought up to white heat by using the blower. Again at noon it was covered with fresh coal and subjected to the blower. At 3 o'clock it was taken from the fire and allowed to cool. No white ash was formed on

it; it retained its form, somewhat shrunken, and lost in weight after six hours firing just 25 per cent.

It is very difficult to mix it properly with sufficient air for complete combustion, as it requires seven to eight times its own volume of air.

There are three ways of getting at this combustion. The first is on the blow-pipe principle, with a strong blast of cold air and a heavy pressure of gas; this is a favorite and a stupid way. The second is what might be called the blow pipe regenerative plan, by the use of a strong flow of gas, and a steam or other blast, driven through the air flues of a regenerative furnace. This produces a furious fire that is pretty to look at, good to stand away from and which must be difficult to work with, besides being very destructive. The third is the regenerative plan pure and simple; it is got at by relieving the gas from pressure, increasing its volume, splitting it up so that air can get to it, and then mixing it with a sufficient quantity of air, as hot as it can be made. The slower and lazier the movements of the gas and air the better, and the result is a beautiful, soft, intense heat, that gives us the greatest amount of work with the least wear and tear. These methods are parallelled in using coal, by the reverberatory style, the blast style, and the regenerative gas style. Except for the necessary use of blast furnaces, the regenerative gas system is incomparably ahead of the others, in both efficiency and economy.

Notwithstanding the doubts of the so-called conservative men, it is a fact that 2,240 pounds of muck bar can be made in this way with 15 bushels of slack; and the record of a whole year's run, including the drowning out by floods, and the balling up of large quantities of very thin steel scraps, shows that by the most adverse figuring the cost of fuel per 2,240 pounds of product could not be made up to quite 60 cents, and the natural gas men positively refused to include that furnace in an offer to furnish gas for the fuel bill, because they were getting more than twice that sum from the iron mills for their puddling furnaces. If then, by the indirect and expensive process of splitting up solid coal in a gas producer into a gas of the average composition of 70 per cent. nitrogen, 10 per cent. carbonic acid, and 20 per cent. carbonic oxide, we can obtain the great economies which we have already secured, we ought certainly to be able to do much better with natural gas which is all combustible. To change a regenerative furnace over to the use of natural gas is a very simple matter; it is only necessary to use one-fifth of the volume required of producer gas, to relieve it of pressure, split it up, and mix it with five times as much air per volume as the producer gas required. This air is obtained in a continuous regenerative furnace by supplying the one-fifth volume of natural gas to the gas ports and using the ordinary supply of air, the equation then reads: Producer gas (one-fifth combustible) + air, one-fifth natural gas (all combustible) + air. Practice shows that that equation is wrong, and the expression should be: Producer gas + air, less than one-fifth natural gas + air; because there is a large gain in effective heat due to the absence of the four-fifths of non-combustible gases which the producer makes, and which have to be kept up to the temperature of the furnace. In the Siemens regenerative furnace, the natural gas is applied cold into the ports, and all of the chambers are used for heating the air. This looks like a perfect arrangement, and it comes very near it in

our present state of knowledge; but it is conceivable if not possible, that an absorbent of nitrogen might be discovered, which would allow us to have a supply of pure oxygen to apply to our natural gas; that would make combustion perfect.

The different modes of burning this gas may all be seen in operation in the city. On the South Side, boilers are fired on the continuous regenerative plan, and the working is very beautiful. The men in charge will tell you that the combustion is perfect, and in proof of it, will open a side door near the rear of the boiler to let you see for yourself. If then you happen to have your eyes in your head, and observe that the moment the door is opened and more air is admitted, a new volume of white flame is formed that curls gracefully off into the flues, it will not be necessary to offend your polite host by casting doubts on the perfection of his combustion.

Up the Allegheny river you may see the blow pipe plan applied to boilers in all of its hideous perfection. It is the joint invention of some gas fiend and some impecunious boiler maker. It may be relied upon to rip up a set of boilers every year and to waste far more heat than it utilizes. Intermediate between these extremes, there are numbers of different appliances in use; the users are generally very kind in showing them and each is undoubtedly the best in use; a walk through the town then will be productive of much valuable information.

Now we must consider what interest this society has in the matter. It would be superfluous to repeat the causes of Pittsburgh's growth and supremacy in the past; we know the story, and we know that as other coal fields have been explored, each new region has announced the immediate downfall of Pittsburgh; and while we have laughed at such vain boasting, we know that we have many powerful competitors outside of the limits of Western Pennsylvania, and that we dare not be wasteful, nor depend too much upon the rule of thumb any more.

But just at the time when our people are beginning to be anxious, nature turns out another great boon from her bounteous lap, in the shape of the best and handiest, and what ought to be the cheapest fuel in the world; and if the members of this society do not join hands with every worker in the town, and proceed to make this the greatest and most prosperous manufacturing center in the world, tariff or no tariff, then we deserve to have this gas mixed with thirteen volumes of air, so that the whole concern may be blown into everlasting oblivion. Then again the chance of getting rid of our horrid soot and smoke, is too good to be lost, and the gain to health, comfort, and universal happiness, is hardly calculable. In addition, it will prove to be an illuminant, superior to anything we have now. It will be cheaper than manufactured gas, and cheaper and handier, if not brighter than the electric light. It burns beautifully in a Siemens regenerative lamp, and will undoubtedly be very much improved by experience.

Our first interest is to learn to use this gas economically and without pressure; there is no economy in the gas companies in either pressure or price. One gas man was asked why he did not sell his gas at so much per pound, per hour, per square inch of orifice? He replied that that was the only way to sell it, and if the party would buy it that way he would bring

him a proposition in forty-eight hours; he was told to do so, and departed. In two or three days he returned with his old style of prices, moderated just enough to induce his customer to take hold, and said nothing about the orifice business. When he was reminded that he had promised another sort of bid, he said, "yes I know I did, but I couldn't figure it out." There is a chance for some of our mathematical friends to do some figuring.

The roarers now supply our mills through six inch and eight inch pipes; the time will come when these mains will be used through the country to gather the supplies from the little permanent wells, and twenty-four to thirty-six, or even forty-eight inches, will be the measure of the diameter of the city mains; probably large gasometers will be built, and exhaust fans will be used to coax our subtile benefactor from his hiding places in the holes and crannies of the rocks. Having once tasted of the benefits of this gas, we will surely never give it up. Now, it is a pertinent question to ask, if such great economy is to be had in the use of regenerative gas furnaces, particularly in puddling, why have not our iron manufacturers adopted them? The answer is, it is the fault of the engineers, or the want of engineers. Some of the iron manufacturers have tried them to their cost; in some cases, the designers have made ridiculous changes from successful forms, just to show their inventive genius, and the changes have proved disastrous. Cases could be cited where thousands of dollars were lost from this cause. In other cases, the designs were all right but the furnaces were turned over to the puddlers, and they soon condemned them because they did not understand them. It will not do to blame the puddlers, for men do not hire out a large surplus of brains at three dollars a day, when their time is fully occupied in distressing labor. This point was aptly illustrated by our lamented friend Holley, who made an engagement, at a handsome salary, to give all of his time to the great Bessemer company. When he invented the movable shell for basic converters, the company claimed the United States patent, but he at once sent in his resignation and exclaimed indignantly to a friend, that he "had sold his time but he had not sold his brains." The resignation was not accepted, and fifty thousand dollars was paid to his family for the patent. The very least use in the world that a manufactory has for an engineer, is in the designing and constructing of plant; that is important work of course, but the true work of the manufacturing engineer is to go down to that plant, and stay with it day and night, until he has patiently and pleasantly instructed everybody in its use. This too is one of the pleasantest occupations of an engineer, if he only has a little patience, and the courage to admit having made a blunder the moment he sees it. He can then command the willing help and sympathy of all of the hands about him. It is true that he is the best engineer who makes the fewest mistakes, but he is only a fool who never makes a blunder.

The old cry, that ignorant and stubborn workmen by their obstinacy, and timid capitalists by their meanness, obstruct improvements, is all bosh. Capitalists expect designers and inventors to show that their designs and inventions are improvements, and in that they are wise. The men who have to work the improvements expect to be instructed in every detail of their working, and in that they are equally wise. In an exper-

ience of nearly twenty-seven years in our foundries and mills, I have been taught some startling and humiliating lessons by members of the horny handed fraternity, and have been led to the conclusion that there is often a good hard brain guiding a good hard hand. The sum of it all is, that it is wise for a man to be very quiet in both office and mill, unless he knows precisely what he is about; and if he does know what he is doing, he will have no lack of able and intelligent assistance.

DISCUSSION.

Mr. Miller.—It is a very useful paper, and I hope it will be well discussed.

Mr. Jarboe.—Mr. Metcalf, will you please tell me what sort of a furnace this piece of coke came from?

Mr. Metcalf.—It was formed on the bridge wall of a Swindell furnace. There is no blackboard here, and I cannot well explain without. The gas was put in the ordinary gas port of the furnace in such a way as to deprive it of all pressure. The furnace was working beautifully at the time, but as the bridge walls got hot, got up to a white heat, the gas in flowing over them, deposited this carbon. If allowed to remain, it actually would have filled the furnace up.

Mr. Jarboe: I presume it was the same as the gas in a retort. It was decomposed in passing over the hot wall.

Mr. Metcalf: Yes, sir, and deposited this carbon. I presume it is well known as a feature of this gas that it can not be superheated. When mixing with air it is necessary to get the gas in cold.

Mr. Lowry: What kind of a furnace was it?

Mr. Metcalf: It was a Swindell regenerative furnace.

Mr. Jarboe: Mr. Metcalf, did that furnace smoke at all?

Mr. Metcalf: It smoked worse than any boiler stack you ever saw, if you put in too much gas. I believe they are arranging them now so as to be smokeless.

Mr. Jarboe: There are two of those furnaces on the South Side doing good work, so the people that own them say.

Mr. Metcalf: They ought to shut the gas off a little, and then they won't smoke.

Mr. Harlow: Mr. President, there is one point I would like to mention. It is a point I do not know that anything can be done for, but it seems to me one of considerable moment, and that is whether you can not control the waste of the gas? I was in Wellsburg, W. Va., not long ago, and noticed in a well there that the gas was light in pressure. They said that well was giving out and that wells a short distance off were wasting the gas. I noticed that some wells were burning too, and it seems to me that in some way the waste should be prevented.

Mr. Jarboe: At the present time more gas is being wasted within 22 miles of Pittsburg than is being used to-day. There is, on a close estimate, 65,000,000 to 70,000,000 feet of gas going to waste in the Murraysville district. Take for instance the Verner well, the Hukill well on the McWilliams farm and the Hukill well on the Daum farm. There are three large and one small well going to waste. There are three large wells blowing the

gas to waste in Washington county. There are three large and one small well going to waste in the Tarentum districts to-day. One there giving out the gas at a pressure of 15 to 17 pounds, with the casing wide open.

Now, in the oil country they have a law requiring them to plug the hole when they are not using it. I think that the engineers and the people of Pennsylvania, Western Pennsylvania, should induce the Legislature, for they are the ones interested in that question, to pass a law requiring people who own wells, if they do not have any use for the gas, to plug the well.

Wells are drilling every day and this waste is expensive. I know a well that was gauged by the same gauge a year ago this month to fully 10 ounces pressure, mercury pressure. It blew 10 ounces through a 5⅝ casing, no salt, no incrustation in it at all. That well to-day is not blowing more than 8 or 8½ ounces. It is a very little difference you may say in a year but still that makes a big difference in the amount of gas that it gives out.

The Westinghouse well No. 1 blew at about 15 pounds when first opened and in 21 days it decreased 60 per cent. Now, I think that if some law can be passed requiring the people who drill wells and don't have any use for the gas after they get it, or any place to put it, to plug them up, it would be a great benefit.

A well can be plugged. Mr. Westinghouse told me he drove a plug in his well and held it a long time. Just think of it! The idea of blowing 50,000,000 cubic feet of gas away in a day right along, and then complaining that our competitors are selling iron cheaper than we can make it, and we not using this gas!

I think the manufacturers here ought to take some interest in the matter and try to get a law passed and hold to it, compelling people to plug wells.

Mr. Roberts: I understand that a few days ago a gas well, a weak one with small discharge near Belle Vernon, 40 miles above this city, on the Monongahela river, had been considerably improved in its yield by means of blasting—some 50 quarts of nitro-glycerine having been exploded in it at the depth of 1,940 feet. One care in the case of wells, at least about Pittsburgh, that has to be taken in blasting, is to make sure that the gas rock stratum is not so much shattered as to admit the water from the salt water veins below. For in case the water cannot be shut off by means of a plug the well is soon ruined. I am inclined to think that in many instances that so-called "failing wells" fail because the pores of the rock become choked with incrustations of salt and "gas dust." Blasting with light charges in this case might be advantageous, as a greater area with new pores would present themselves for the exit of the gas. The gas would certainly follow the cracks created by the explosion, and so come to the surface.

Mr. Boyd: I was going to ask Mr. Jarboe whether he thought it possible to plug a well like the Hukill well on the Dunn farm. I do not think you could.

Mr. Jarboe: I think it could be plugged.

Mr. Boyd: Have you been there?

Mr. Jarboe: Yes sir. I would undertake to plug it. I have seen wells plugged that blew 17 pounds solid pressure through a 5⅝ casing.

Now, in regard to blasting, as Mr. Roberts spoke about. I have just finished a little diagram for the government and had to make some experiments to find some things out. In the bottom of a gas well, when the hole commences to choke, they have blasted with nitro-glycerine. Pieces of the rock had been brought up to the surface, and on the pieces you find little barnacles, or rather a substance looking like barnacles. You see a large hole next to the rock and a little lower another one somewhat smaller, and these get smaller and smaller until it forms a cone, and the last layer of that cone closes it up entirely. It was analyzed by the United States chemist at Washington.

When they blast these wells and are not troubled with salt water they do improve for quite a length of time. Also in some of these wells that get plugged up we find in the rock next to the shell that these holes are plugged up completely with paraffine to a depth of at least 2½ inches. That is the largest piece I ever got out of a hole. I have a piece of rock filled to a depth of nearly 2½ in. with paraffine, that is from the surface of the rock. The well it came from was vastly improved by blasting.

Now, I know of two wells that are a good ways apart, one up in the eastern belt of McKean county where the paraffine choked it. The other well was where they have no oil. Both of these wells were improved by blasting and yet neither one of them was troubled with salt water.

But in all this country, right underneath our gas veins there is an ocean of salt water, known among drillers as the Atlantic Ocean. The reason it is so called is that the analysis of it shows very near the same analysis as the water of the Atlantic Ocean itself. There is no way when you get that broken into with a torpedo of stopping it. The blasting breaks the rock up so that it gets beyond plugging.

Mr. Lowry: If these people in the gas offices know all about it and want to save the gas, why don't they get a bill through the Legislature to have these wasting wells plugged. They are the ones most interested and that is the way to stop it.

Mr. Thomas N. Miller: I think the Society should take some action on that subject. I know of a well out near Wellsburg, W. Va., an enormous one, that has been burning for years. It is burning gas at an estimate rate of $1,200 per day. It seems to me like tremendous arson to allow this to go to waste. I think some action ought to be taken tending toward legislation to prevent such waste.

Mr. Boyd: How would it do for drillers, where they have no use for the gas when they obtain it, to stop drilling before they get down to the gas vein and then leave the well until they are ready to use the gas. It amounts to the same thing as plugging and saves trouble. Then they could tap the vein and let the gas out.

Mr. William Miller: Could not some of our mechanical engineers invent some sort of a tap pipe with a sliding valve on it that could be left out when the gas was struck so that the pipe would be enabled with the valve to prevent the waste. I would refer that to some of our mechanical engineers to think about.

Mr. Jarboe: That has been tried and the only success it met with was to bore a hole through the top of the derrick. And as to my friend from McKeesport who spoke about stopping when the drill struck sand, I

would say that that can only be done on developed territory. But people go out prospecting for this gas, wild-catting, and when drilling it would be ticklish to stop in that manner. You would not know definitely whether there was sand or not. They could not develope the territory if they did that. People go into a territory to strike gas with no idea what they can do with it after they get it. They are miles away from any place where it can be used and it would take thousands of dollars to bring the pipes for it into paying territory. And even after they get a well down and make a strike they have no money to take it away. Here they have struck perhaps an immense well and the gas is going to waste to the detriment of other wells and of other people who would like to get the gas so much.

They ought not to be allowed to drill for gas under such circumstances and if they do strike it they should be made to plug the well. Wells have been plugged many and many a time.

Mr. William Miller: I think there are mechanical brains in Pittsburgh, certainly in the country, to make a valve which can be utilized when a gas well is drilled and gas is struck, so as to prevent it going out at the top of the derrick. You say you can plug a well. Why could not the valve be made to do the work without that plugging?

Mr. Jarboe: All the valves that have been made so far have been for the casing.

Mr. William Miller: Well, put it below the ground. Have a very heavy pipe for the top and the valve so arranged as to use it or not, as may be required. I think that any person who owns a gas well would be as much put out at seeing the gas go to waste as his neighbor, who is not particularly interested. I presume, however, that could be overcome by plugging.

Mr. Lowry: I don't see the use particularly of talking about this matter. If people who own wells don't want them plugged, so as to prevent waste, it seems to me there is no particular use for engineers to try to get the Legislature to compel them to do it.

Mr. Brashear: I was much interested in Mr. Metcalf's paper. I think there is nothing more interesting to discuss in this Society than the use of natural gas. Talking about stopping wells reminds me of a story they tell about our friend Mr. Ricketson. When his attention was called to a blow hole in a chill roll, he said "pene it, pene it, and if you cannot pene it, plug it." It may be that the same process would do with wells, but I think that question is hardly the one for us to discuss so much really as the use of it. As it is, if any parties are to be shut up they must be shut up by proper legislation.

On my travels in the east lately I found that the people are particularly interested in the discussions that have been going in this Society, especially the first paper read before the National Association, *i. e.* on natural gas. I found a wonderful interest among the eastern people on this question and that is one reason why I think the paper should be published.

While I am speaking I will just say that the Boston Journal of Science, or rather the journal called "Science" is asking for briefs of meetings of societies of this kind. I saw in this weeks paper a report or abstract of

papers read at the Engineer's Association in Philadelphia. I think that Pittsburgh is little known to what it should be, and I think that the Engineers' Society of Pittsburg, in their work and in their discussions, especially on natural gas, should make themselves better known. Their reports and papers are of value and would be very valuable and very interesting to engineers outside of Pittsburgh.

There is so much in this question. To me the interesting point is its relations to the manufacture of glass, especially that used in instruments of precision. Now I believe that the time is coming when natural gas will be largely used in this work. I believe that the question of annealing the glass, which is so difficult and which has caused more trouble probably than any other thing, will be settled by this question of natural gas because it can be so utilized that it can be shut off nicely, so easily, so gradually, as to let the molecules of the glass come to their normal position without strain.

I have a little piece of glass in my pocket to-night, annealed at a factory in Tarentum, and I can detect no strain in it whatever. Prof. Rogers has ruled upon it 10,000 lines, and has sent it to me to give to Mr. Metcalf as a relic of a visit to that works. That is one of the good uses of natural gas.

When I get a little better off in the world, I want to see if I cannot get a pipe laid up to my shops and I shall certainly experiment in the annealing of glass, to get it into its normal condition. The trouble has been getting the material homogeneous. I have never yet seen an absolutely homogeneous piece of material, and if we can make it in Pittsburgh we shall have a little name and some honor outside of our own country.

Mr. Metcalf: I will mention another application of this gas that I heard of the other day. I did not include it in my paper because I had no opportunity of seeing it for myself. It is certainly very peculiar and if it is anything like what is reported, it is another adaptation of the gas. It is in pickling, or rather doing away with. pickling. All who are familiar with the manufacture of very thin sheets of metal, either iron or steel, know the great difficulty there is in pickling the scale off in order to get a fine finished surface. They know the danger of the acid penetrating through the metal and destroying it. It is a difficult thing to do well, and the operation is one that must be done carefully and is one that everybody that has it to do will be glad to get rid of.

I am told that a gentleman at Leechburg, now applying for a patent on the process, in annealing fine sheets, brings the annealing box up to the required heat by use of the natural gas, and then by a pipe, connected into the box, when the metal is hot enough, turns in a stream of the natural gas on to the material and allows it to pass through, keeping the box hot for some little time, and then allowing it to cool gradually, when the whole mass of sheets come out perfectly clean, as clean as tin, but not as bright, but entirely clean; and singular to say that though the sheets are very thin and packed closely in heavy boxes this gas in some way gets in all among them and they come out perfectly clean and free from scale.

This is another application of the gas which will certainly be of great benefit if it proves to be practicable, and there is no reason why it should not be.

MR. HENNING: The principal trouble and objection now met with in annealing bridge members is the great amount of rust which collects on material in ordinary annealing furnaces, and this discovery, described by Mr. Metcalf, would prevent the difficulty entirely.

MR. JARBOE: I have heard of the process spoken of by Mr. Metcalf and think I spoke of it once before. I saw some of that iron, in fact have a piece of it that was cold-rolled after being taken out of the box. It looks very much like a good quality of tin only without the bright quality. It looks like dead tin. It seems under the microscope before re-roiling to be full of little cells, very close together where the oxide has gotten into it and been eaten out by the gas. It rolls much thinner.

After going through this process it will take a good deal of use before it will show much rust. I have tried a piece and could not rust it easily.

Further discussion was postponed until the next meeting, the hour being late.

CRITICAL METHODS OF DETECTING ERRORS IN PLANE SURFACES.

BY JOHN A. BRASHEAR.

[A paper read before the Engineers' Society of Western Pennsylvania, Dec. 16, 1884.]

In our study of the exact methods of measurement in use to-day, in the various branches of scientific investigation, we should not forget that it has been a plant of very slow growth, and it is interesting indeed to glance along the pathway of the past to see how step by step our Micron of to-day has been evolved from the cubit, the hands breadth, the span, and if you please, the barley corn of our schoolboy days. It would also be a pleasant task to investigate the properties of the gnomon of the Chinese, Egyptians, and Peruvians, the scarphie of Eratosthenes, the Astrolobe of Hipparchus, the parallactic rules of Ptolemy, Regimontanus Purbach and Walther, the sextants and quadrants of Tycho Brahe, and the modifications of these various instruments, the invention and use of which, from century to century, bringing us at last to the telescopic age, or the days of Lippershay, Jannsen and Gallileo. It would also be a most pleasant task to follow the evolution of our subject in the new era of investigation ushered in by the invention of that marvellous instrument, the telescope, followed closely by the work of Kepler, Scheiner, Cassini, Huyghens, Newton, Digges, Nonius, Vernier, Hall, Dollond, Herschel, Short, Bird, Ramsden, Troughton, Smeaton, Frauenhoffer, and a host of others, each of whom has contributed a noble share in the elimination of sources of error, until to-day we are satisfied only with units of measurements of the most exact and refined nature. Although it would be pleasant to review the work of these past masters, it is beyond the scope of the present paper, and even now I can only hope to call your attention to one phase of this important subject. For a number of years I have been practically interested in the subject of the production of plane and curved surfaces particularly for optical purposes, *i. e.*, in the production of such surfaces, free if possible from all traces of error, and it will be pleasant to me if I shall be able to add to the interest of this association by giving you some of my own practical experience, and may I trust that it will be an incentive to all engaged in kindred work *to do that work well.* In the production of a perfectly plane surface, there are many difficulties to contend

with, and it will not be possible in the limits of this paper to discuss the methods of eliminating errors when found, but I must content myself with giving a description of various methods of detecting existing errors in the surfaces that are being worked, whether, for instance, it be an error of

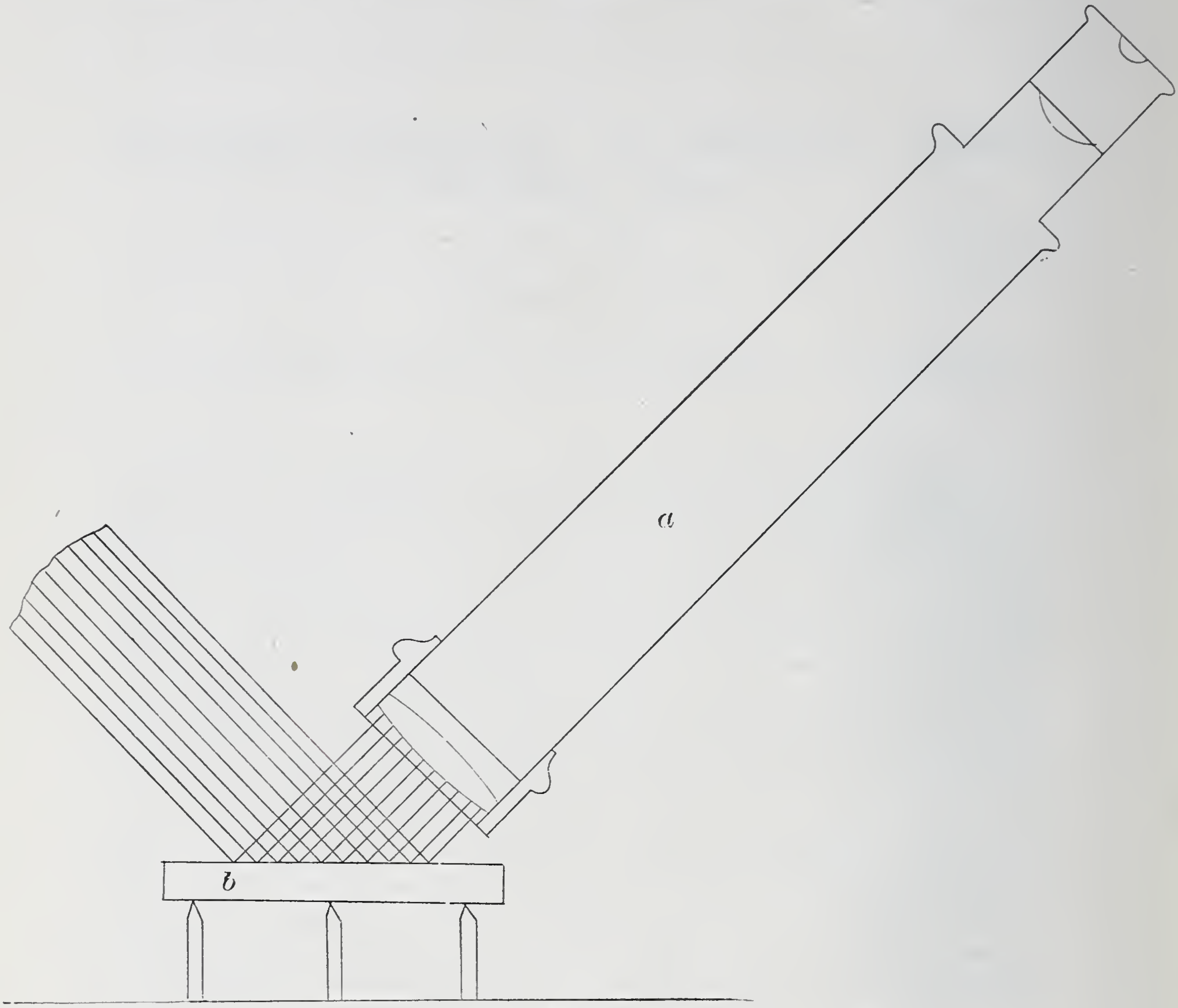


FIG. 1.

concavity, convexity, periodic or local error. A very excellent method was devised by the celebrated Ross, which is frequently used at the present time, and those eminent workers, the Clarks of Cambridge, use a modification of the Ross method which in their hands is productive of the very highest results. The device is very simple, consisting of a telescope (*a* Fig. 1) in which the aberrations have been well corrected so that the focal plane of the objective is as sharp as possible. This telescope is first directed to a distant object, preferably a celestial one and focused for parallel rays. The surface *b* to be tested is now placed so that the reflected image of the same object whatever it may be, can be observed by the same telescope. It is evident that if the surface be a true plane, its action upon the beam of light that comes from the object will be simply to

change its direction, but not disturb or change it any other way, hence the reflected image of the object should be seen by the telescope *a* without in any way changing the original focus. If, however, the supposed plane surface proves to be *convex*, the image will not be sharply defined in the telescope until the eye piece is moved *away* from the object glass, while if the converse is the case and the supposed plane is concave, the eye-piece must now be moved *toward* the objective in order to obtain a sharp image, and the amount of convexity or concavity may be known by the change in the focal plane. If the surface has periodic or irregular errors, no sharp image can be obtained, no matter how much the eye piece may be moved in or out. This test may be made still more delicate by using the observing telescope *a* at as low an angle as possible, thereby bringing

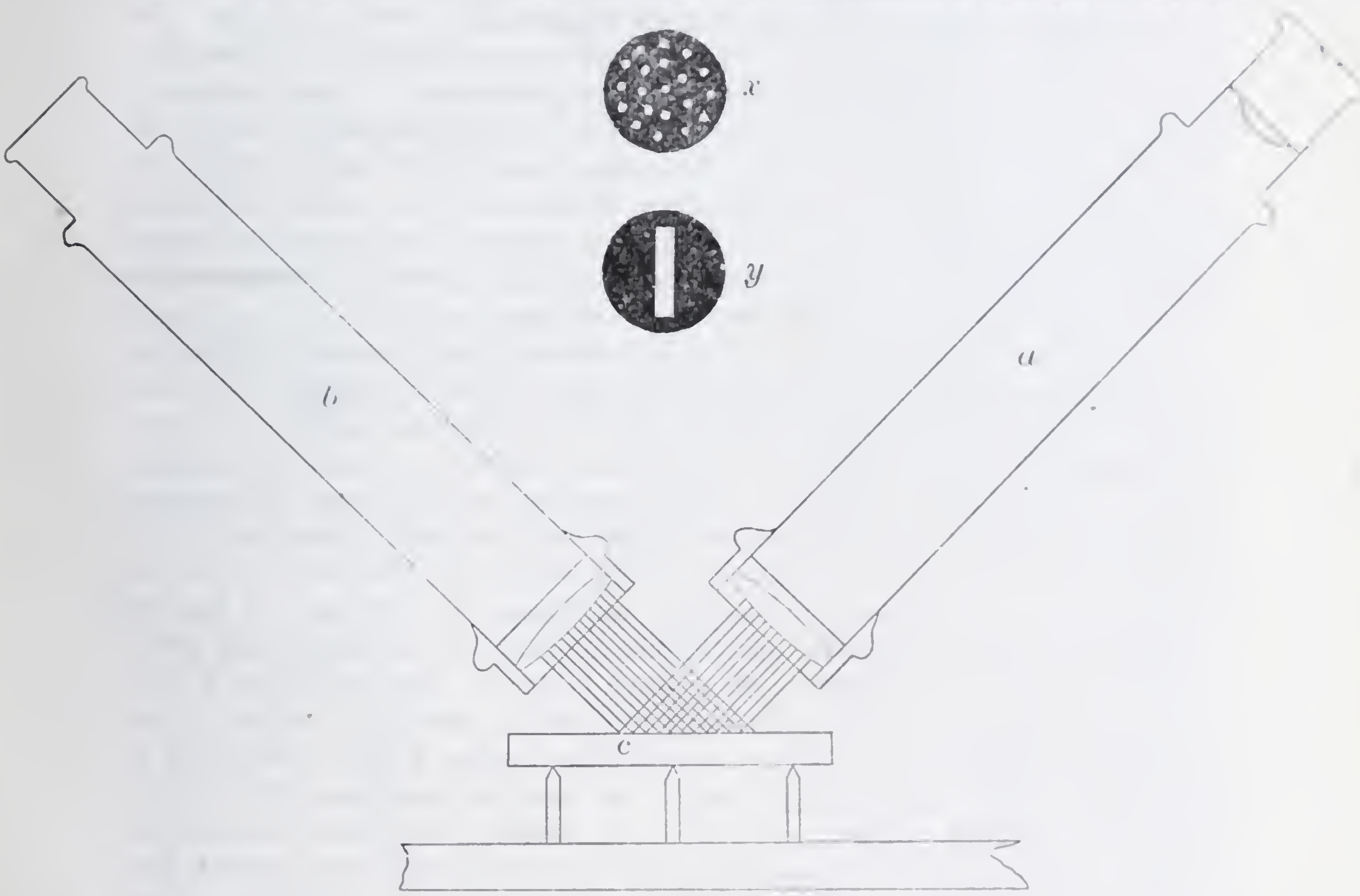


FIG. 2.

out with still greater effect, any error that may exist in the surface under examination, and is the plan generally used by Alvan Clark & Sons. Another and very excellent method is that illustrated in Fig. 2 in which a second telescope *b* is introduced. In place of the eye piece of this second telescope, a diaphragm is introduced in which a number of small holes are drilled as in Fig. 2, *x*, or a slit is cut similar to the slit used in a spectroscope as shown at *y* same figure. The telescope *a* is now focused very accurately on a celestial or other very distant object, and the focus marked. The object glass of the telescope *b* is now placed against and "square" with the object glass of telescope *a* and on looking through telescope *a* an image of the diaphragm with its holes or the slit is seen. This diaphragm must now be moved until a sharp image is seen in telescope *a*.

The two telescopes are now mounted as in Fig. 2, and the plate to be tested placed in front of the two telescopes as at *c*. It is evident as in the former case that if the surface is a true plane, the reflected image of the holes or slit thrown upon it by the telescope *b* will be seen sharply defined in the telescope *a*. If any error of convexity exists in the plate, the focal plane is disturbed and the eye piece must be moved *out*. If the plate is concave it must be moved *in* to obtain a sharp image. Irregular errors in the plate or surface will produce a blurred or indistinct image, and as in the first instance no amount of focusing will help matters. These methods are both good, but are not satisfactory in the highest degree, and two or three important factors bar the way to the very best results. One is that the aberrations of the telescopes must be perfectly corrected, a very difficult matter of itself, and requiring the highest skill of the optician. Another, the fact that the human eye will accommodate itself to small distances when setting the focus of the observing telescope. I have frequently made experiments to find out how much this accommodation was in my own case and found it to amount to as much as $\frac{1}{40}$ of an inch. This is no doubt partly the fault of the telescopes themselves but unless the eye is rigorously educated in this work, it is apt to accommodate itself to a small amount and will invariably do so if there is a preconceived notion or bias *in the direction of the accommodation.* Talking with Prof. C. A. Young a few months since on this subject, he remarked that he noticed that the eye grew more exact in its demands as it grew older, in regard to the focal point. A third and very serious objection to the second method is caused by diffraction from the edges of the holes or the slit. Let me explain this briefly. When light falls upon a slit, such as we have here, it is turned out of its course; as the slit has two edges and the light that falls on either side is deflected both right and left, the rays that cross from the right side of the slit toward the left, and from the left side of the slit toward the right, produce interference of the wave lengths, and when perfect interference occurs, a dark line is seen. You can have a very pretty illustration of this by cutting a fine slit in a card and holding it several inches from the eye; when the dark lines cause a total extinction of the light by interference may be seen. If now you look toward the edge of a gas or lamp flame you will see a series of colored bands, that bring out the phenomenon of partial interference. This experiment shows the difficulty in obtaining a perfect focus of the holes or the slit in the diaphragm, as the interferences fringes are always more or less annoying. Notwithstanding these defects of the two systems I have mentioned, in the hands of the practical workmen, they are productive of very good results, and very many excellent surfaces have been made by their use, and we are not justified in ignoring them, because they are the stepping stones to lead us on to better ones. In my early work Dr. Draper suggested a very excellent plan for testing a flat surface, which I will briefly describe. It is a well known truth that if an artificial star is placed in the exact centre of curvature of a truly spherical mirror, and an eye piece be used to examine the image close beside the source of light, that the star will be sharply defined and will bear very high magnification. If the eye piece is now drawn toward the observer the star disc begins to expand, and if the mirror be a truly spherical one the expanded disc will be equally illuminated except the

outer edge which usually shows two or more light and dark rings due to diffraction, as already explained. Now if we push the eye piece toward the mirror the same distance on the opposite side of the true focal plane, precisely the same appearance will be noted in the expanded star disc. If we now place our plane surface anywhere in the path of the rays from the great mirror, we should have identically the same phenomena repeated. Of course it is presumed and is necessary that the plane mirror shall be much less in area than the spherical mirror, else the beam of light from the artificial star will be shut off, yet I may here say that any one part of a truly spherical mirror will act just as well as the whole surface, there being of course a loss of light according to the area of the mirror shut off. This principle is illustrated in Fig. 3 where a is the spherical mirror, b the source of light, c the eye piece as used when the plane is not interposed, d the plane introduced into the path at an angle of 45° to the central beam, and e the position of eye piece when used with the plane. When the plane is not in the way the converging beam goes back to the eye piece c. When the plane d is introduced the beam is turned at a right angle and if it is a perfect surface, not only does the focal plane remain exactly of the same length but the expanded star discs are similar on either side of the focal plane. I might go on to elaborate this method to show how it may be made still more exact, but as it will come under the discussion of spherical surfaces I will leave it for the present. Unfortunately for this process, it demands a large truly spherical surface which is just as difficult of attainment as any form of regular surface. We come now to an instrument that does not depend upon optical means for detecting errors of

FIG. 3.

surface, namely the spherometer, which as the name would indicate means sphere measure but it is about as well adapted for plane as it is for spherical work, and Prof. Harkness has been using one for some time past in determining the errors of the plane mirrors used in the transit of Venus photographic instruments. At the meeting of the American Association of Science in Philadelphia there was quite a discussion as to the relative merits of the spherometer test and another form which I shall presently mention, Prof. Harkness claiming that he could, by the use of the spherometer detect errors bordering closely on one-five hundred thousandth of an inch. Some physicists express doubt on this, but Prof. Harkness has no doubt worked with very sensitive instruments and over very

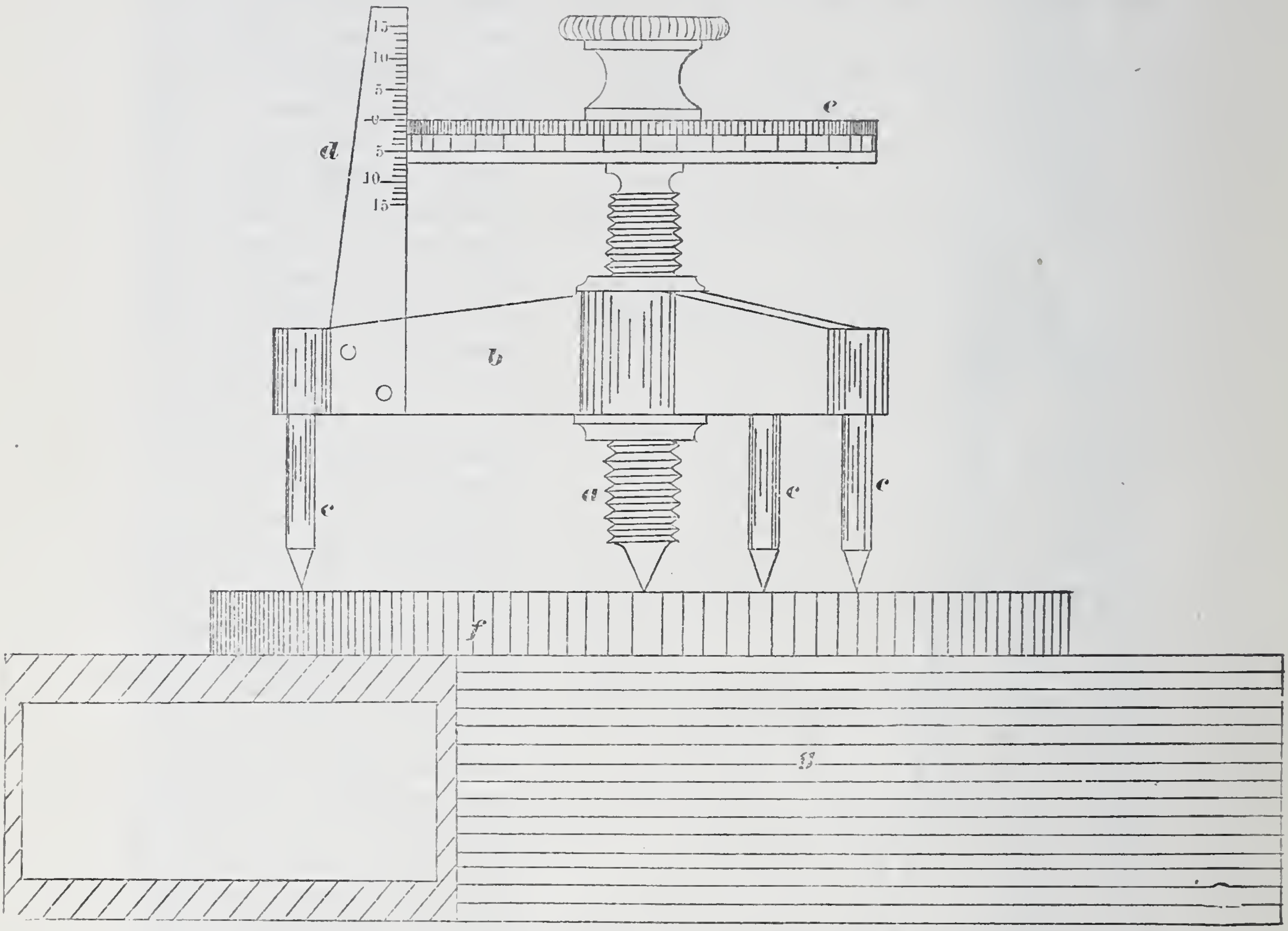


FIG. 4.

small areas at one time. I have not had occasion to use this instrument in my own work, as a more simple, delicate and efficient method was at my command, but for the measurement of convex surfaces I know of nothing that can take its place. I will briefly describe the method of using it. The usual form of the instrument is shown in Fig. 4. *a* is a steel screw working in the nut of the stout tripod frame *b*. *c c c* are three legs with carefully prepared points; *d* is a divided standard to read the whole number of revolutions of the screw *a*, the edge of which also serves the purpose of a pointer to read off the division on the top of the milled head *e*. Still further refinement may be had by placing a vernier here. To meas-

ure a plane or curved surface with this instrument, a perpect plane or perfect spherical surface of known radius must be used to determine the zero point of the division. Taking for granted that we have this standard plate the spherometer is placed upon it and the readings of the divided head and indicator *d* noted when the point of the screw *a* just touches the surface *f*. Herein, however, lies the great difficulty in using this instrument, *i. e.* to know the exact instant of contact of the point of screw *a* on the surface *f*. Many devices have been added to the spherometer to make it as sensitive as possible, such as the contact level, the electric contact, and the compound lever contact. The latter is probably the best and is made essentially as in Fig. 5. I am indebted for this plan to Dr. Alfred Mayer. As in the previous figure, *a* is the screw, this screw is bored out and a

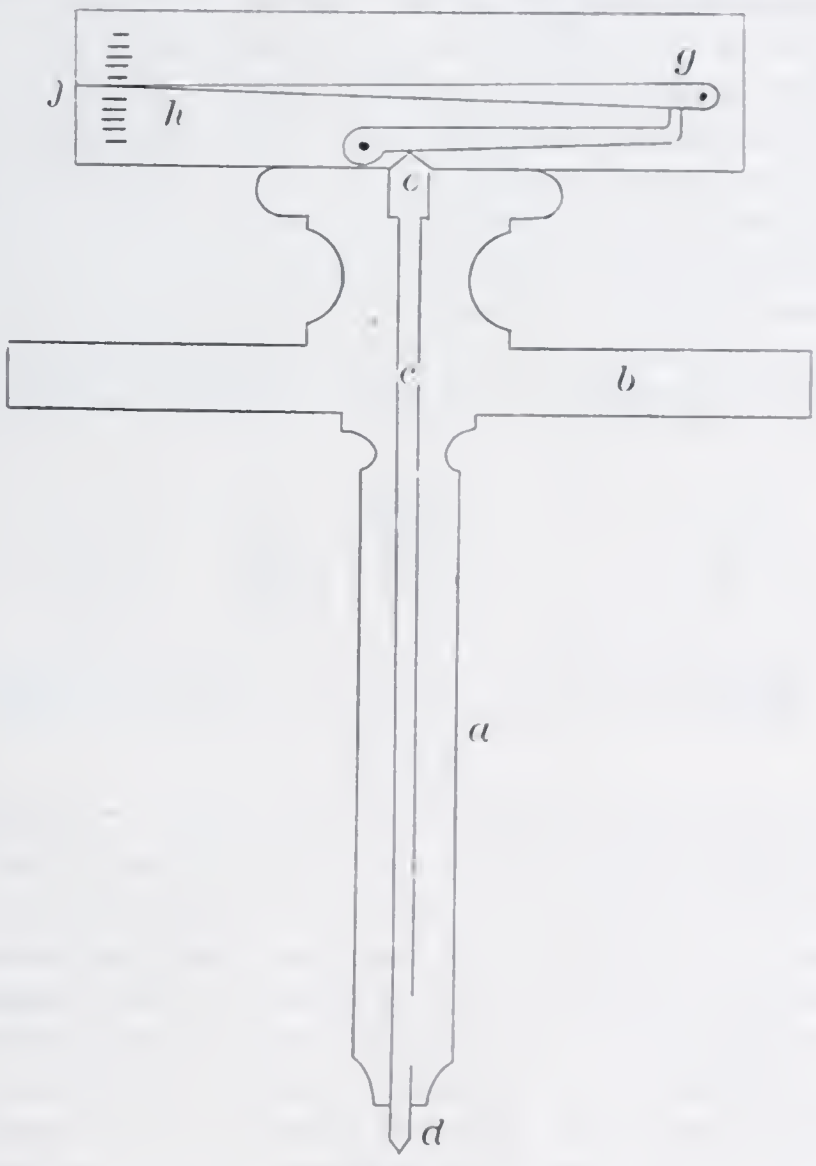


FIG. 5.

central steel pin turned to fit resting on a shoulder at *c*. The end *d* projects below the screw *a* and the end *e* projects above the milled head and the knife edge or pivot point rests against the lever *f* which in turn rests against the long lever *g*, the point *h* of which moves along the division at *j*. It is evident that if the point of the pin just touches the plate no movement of the index lever *g* will be seen, but if any pressure be applied the lever will move through a multiplied arc, owing to the short fulcri of the two levers. Notwithstanding all these precautions, we must also take into account the flexure of the material, the elasticity of the points of contact,

and other idiosyncrasies, and you can readily see that practice alone in an instrument so delicate will bring about the very best results. Dr. Alfred Mayer's method of getting over the great difficulty of knowing when all four points are in contact is quite simple. The standard plate is set on the box *g* Fig. 4 which acts as a resonator. The screw *a* is brought down until it touches the plate. When the pressure of the screw is enough to lift off either or all of the legs and the plate is gently tapped with the finger, a *rattle* is heard which is the tell tale of imperfect contact of all the points. The screw is now reversed gently and slowly until the *moment* the rattle ceases, and then the reading is taken. Here the sense of hearing is brought into play. This is also the case when the electric contact is used. This is so arranged that the instant of touching of the point of screw *a*, completes the electric circuit in which an electro magnet of short thick wire is placed. At the moment of contact or perhaps a little before contact the bell rings and the turning of the screw must be instantly stopped. Here are several elements that must be remembered. First, it takes time to set the bell ringing, time for the sound to pass to the ear, time for the sensation to be carried to the brain, time for the brain to send word to the hand to cease turning the screw, and if you please, it takes time for the hand to stop. You may say, of what use are such refinements? I may reply, what use is there in trying to do anything the very best it can be done. If our investigation of natures profound mysteries can be par-

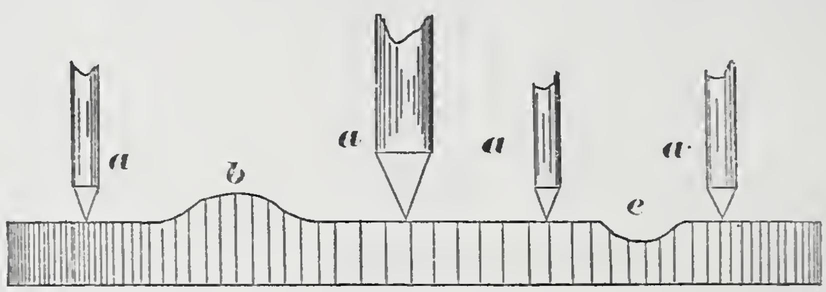


Fig. 6.

tially solved with good instrumental means, what is the result if we have better ones placed in our hands, and what, we ask, if the *best* are given to the physicist. We have only to compare the telescope of Gallileo, the prism of Newton, the pile of Volta, and what was done with them, to the marvellous work of the telescope, spectroscope and dynamo of to-day. But I must proceed. It will be recognized that in working with the spherometer, only the points in actual contact can be measured at one time, for you may see by Fig. 6 that the four points *a a a a* may all be normal to a true plane and yet errors of depression as at *e*, or elevation as at *b*. exist between them, so that the instrument must be used over every available part of the surface if it is to be tested rigorously. As to how exact this method is I cannot say from actual experience, as in my work I have had recourse to other methods that I shall describe. I have already quoted you the words of Prof. Harkness. Dr. Hastings whose practical as well as theoretical knowledge is of the most critical character tells me that he considers it quite easy to measure to $\frac{1}{80000}$ of an inch with the ordinary form of instrument. Here is a very fine spherometer that Dr. Hastings works with from time to time and which he calls his standard spherometer. It is delicately made, its screw being 50 to the

inch or more exactly 0.01998 inch or within $\frac{2}{100000}$ of being $\frac{1}{50}$ of an inch pitch. The principal screw has a point which is itself an independent screw that was put in to investigate the errors of the main screw, but it was found that the error of this screw was not as much as the .00001 of an inch. The head is divided into two hundred parts, and by estimation can be read to $\frac{1}{100000}$ of an inch. Its constants are known and it may be understood that it would not do to handle it very roughly. I could dwell here longer on this fascinating subject, but must haste. I may add that if this spherometer is placed on a plate of glass and exact contact obtained, and then removed and the hand held over the plate without touching it, the difference in the temperature of the glass and that of the hand would be sufficient to distort the surface enough to be readily recognized by the spherometer when replaced. Any one desiring to investigate this subject further will find it fully discussed in that splendid series of papers by Dr. Alfred Mayer on the minute measurements of modern science published in Scientific American Supplements, to which I was indebted years ago for most valuable information, as well as to most encouraging words from Prof. Thurston, whom you all so well and favorably know. I now invite your attention to the method for testing the flat surfaces on which Prof. Rowland rules the beautiful diffraction gratings now so well known over the scientific world, as also other plane surfaces for Heliostats, etc., etc. I am now approaching the border land of what may be called the abstruse in science in which I humbly acknowledge it would take a vast volume to contain all I don't know, yet I hope to make plain to you this most beautiful and accurate method, and for fear I may forget to give due credit, I will say that I am indebted to Dr. Hastings for it, with whom it was an original discovery, though he told me he afterward found it had been in use by Steinheil, the celebrated optician of Munich. The principle was discovered by the immortal Newton, and it shows how much can be made of the ordinary phenomena seen in our every day life when placed in the hands of the investigator. We have all seen the beautiful play of colors on the soap bubble, or when the drop of oil spreads over the surface of the water. Place a lens of long curvature on a piece of plain polished glass, and looking at it obliquely, a black central spot is seen with rings of various width and color surrounding it. If the lens is a true curve and the glass beneath it a true plane these rings of color will be perfectly concentric and arranged in regular decreasing intervals. This apparatus is known as Newton's color glass, because he not only measured the phenomena, but established the laws of the appearances presented. I will now endeavor to explain the general principle by which this phenomena is utilized in the testing of plane surfaces. Suppose that we place on the lower plate, lenses of constantly increasing curvature until that curvature becomes nil, or in other words a true plane. The rings of color will constantly increase in width as the curvature of the lens increases, until at last one color alone is seen over the whole surface, provided, however, the same angle of observation be maintained, and provided further that the film of air between the glasses is of absolutely the same relative thickness throughout. I say the film of air, for I presume that it would be utterly impossible to exclude particles of dust so that absolute contact could take place. Early physicists maintained that abso-

lute molecular contact was impossible, and that the central separation of the glasses in Newton's experiment was $\frac{1}{250000}$ of an inch, but Sir Wm. Thompson has shown that the separation is caused by shreds or particles of dust. However, if this separation is equal throughout, we have the phenomena as described, but if the dust particles are thicker under one

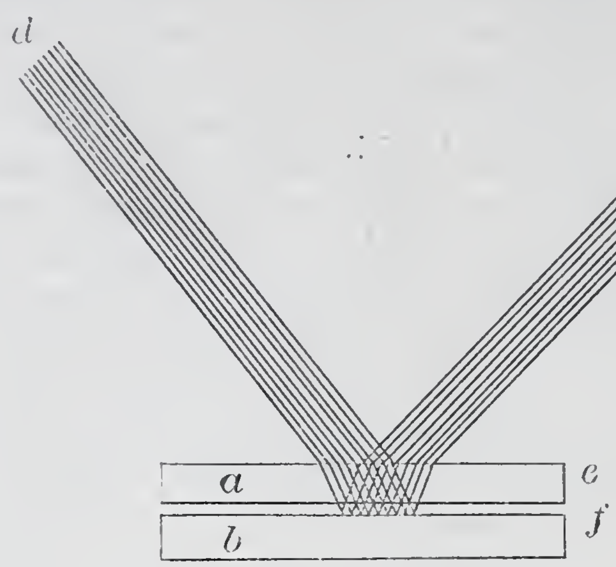


FIG. 7.

side than the other, our phenomena will change to broad parallel bands as in Fig. 8 the broader the bands the nearer the absolute parallelism of the plates. In Fig. 7 let *a* and *b* represent the two plates we are testing. Rays of white light *c* falling upon the upper surface of plate *a* are partially reflected off in the direction of rays *d*, but as these rays do not concern us now I have not sketched them. Part of the light passes on through the upper plate where it is bent out of its course somewhat, and falling upon the *lower* surface of the upper plate some of this light is again

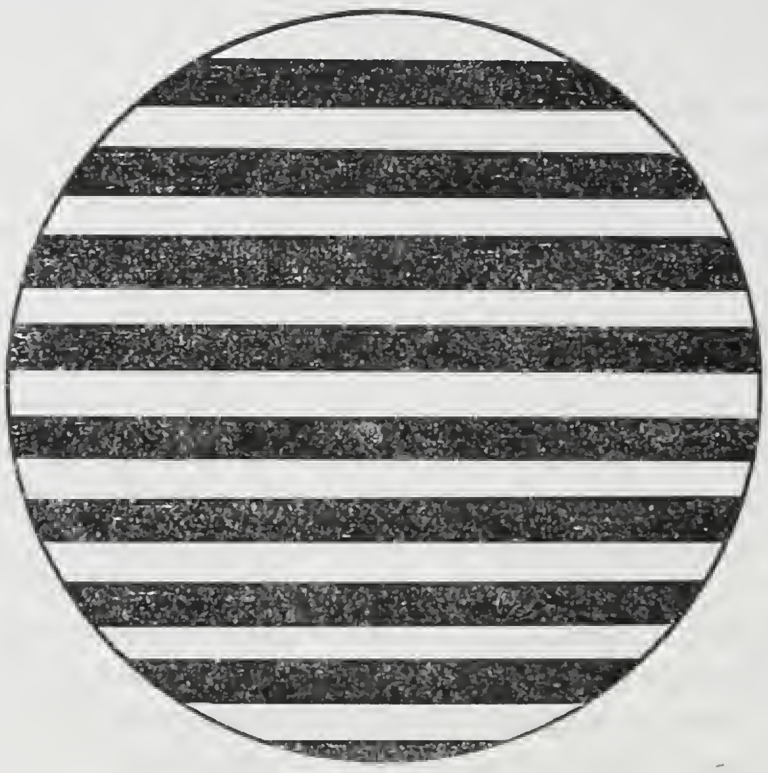


FIG. 8.

reflected toward the eye at *d*. As some of the light passes through the upper plate and, passing through the film of air between the plates,

falling on the upper surface of the *lower* one, this in turn is reflected, but as the light that falls on this surface has had to traverse the film of air *twice*, it is retarded by a certain number of half or whole wave lengths, and the beautiful phenomena of interference takes place, some of the colors of white light being obliterated while others come to the eye. When the position of the eye changes the color is seen to change. I have not time to dwell further on this part of my subject, which is discussed in most advanced works on physics, and especially well described in Dr. Eugene Lommels work on the "The Nature of light." I remarked that if the two surfaces were perfectly *plane* there would be one color seen, or else colors of the first or second order would arrange themselves in

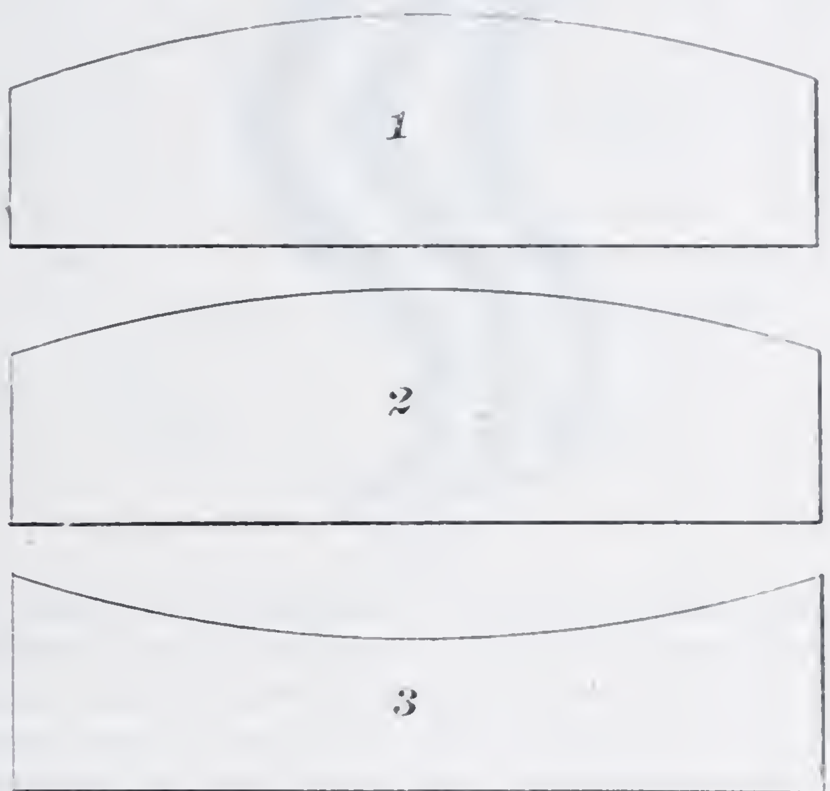


FIG. 11.

broad parallel bands, but this would also take place in plates of slight curvature for the requirement is as I said, a film of air of equal thickness throughout. You can see at once that this condition could be obtained in a perfect convex surface fitting a perfect concave of the same radius. Fortunately we have a check to guard against this error. To produce a perfect plane *three surfaces must* be worked together unless we have a true plane to commence with, but to make this true plane by this method we *must* work three together, and if each one comes up to the demands of this most rigorous test, we may rest assured that we have attained a degree of accuracy almost beyond human conception. Let me illustrate. Suppose we have plates 1, 2 and 3, Fig. 11. Suppose 1 and 2 to be accurately convex and 3 accurately concave, of the same radius. Now it is evident that 3 will exactly fit 1 and 2, and that 1 and 2 will separately fit No. 3, *but* when 1 and 2 are placed together they will only touch in the centre, and there is no possible way to make three plates coincide when they are alternately tested upon one another than to make *perfect planes* out of them. As it is difficult to see the colors well on metal surfaces, a one colored light is used, such as the sodium flame, which gives to the eye in our test, dark and bright bands

instead of colored ones. When these plates are worked and tested upon one another until they all present the same appearance, one may be reserved for a test plate for future use. Here is a small test plate made by the celebrated Steinheil, and here two made by myself, and I may be pardoned in saying that I was much gratified to find the coincidence so nearly perfect

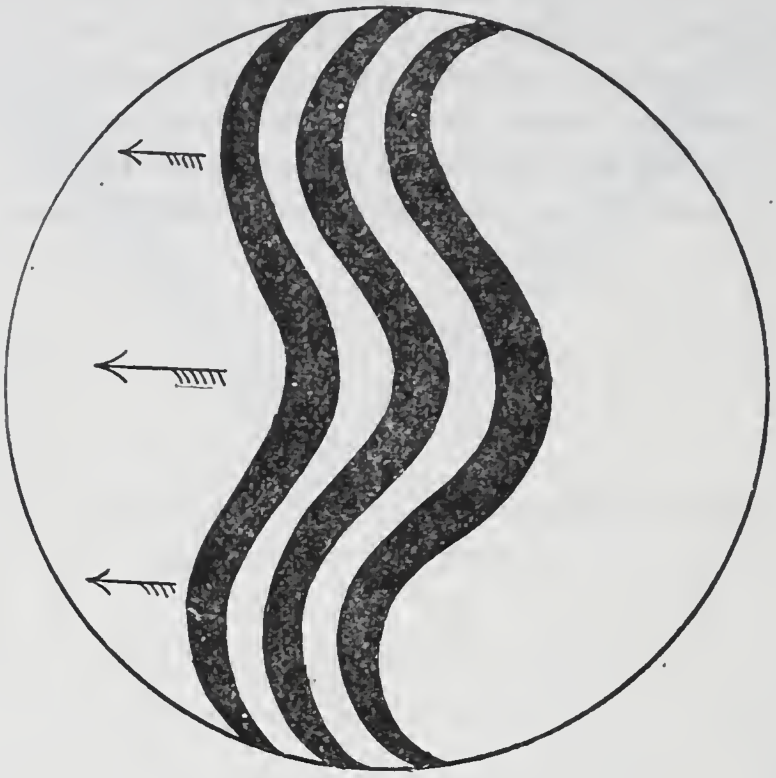


FIG. 9.

that the limiting error is much less than .00001 of an inch. My assistant with but a few months experience has made quite as accurate plates. It is necessary of course to have a glass plate to test the metal plates, as the upper plate *must* be transparent. So far we have been dealing with perfect surfaces. Let us now see what shall occur in surfaces that are not plane. Suppose we now have our perfect test plate, and it is laid on a

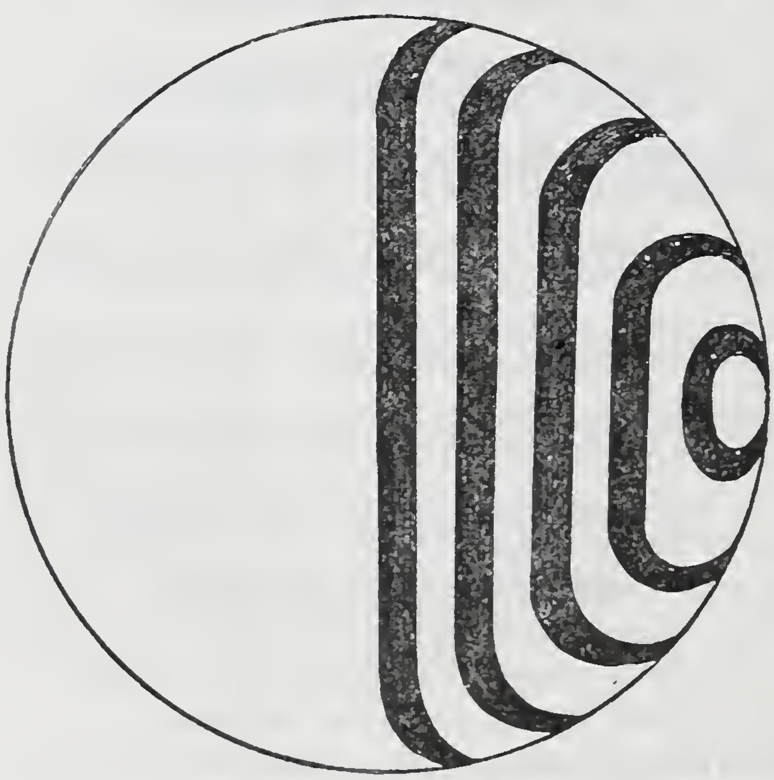


FIG. 10.

plate that has a compound error, say depressed at centre and edge and high between these points. If this error is regular the central bands arrange themselves as in Fig. 9. You may now ask, how are we to know what sort of surface we have. A ready solution is at hand. The bands *always travel in the direction of the thickest film of air*, hence, on lowering the eye, if the convex edge of the bands travel in the direction of the arrow, we are absolutely certain that that part of the surface being tested is convex, while if as in the central part of the bands the concave edges advance, we know that part is hollow or too low. Furthermore, any small error will be rigorously detected, with astonishing clearness, and one of the grandest qualities of this test, is the absence of "personal equasion" for, given a perfect test plate, *it wont lie*, neither will it exaggerate. I say, wont lie, but I must guard this by saying that the plates must coincide absolutely in temperature, and the touch of the finger, the heat of the hand or any disturbance whatever will vitiate the results of this lovely process; but more of that at a future time. If our surface is plane to within a short distance of the edge and is there overcorrected, or convex, the test shows it as in Fig. 10. If the whole surface is regularly convex then concentric rings of a breadth determined by the approach to a perfect plane are seen. If concave, a similar phenomena is exhibited except in the case of the convex, the broader rings are near the center while in the concave they are nearer the edge. In lowering the eye while observing the plates, the rings of the convex plate will advance outward, those of the concave inward. It may be asked by the mechanician: can this method be used for testing our surface plates? I answer, that I have found the scraped surface of iron bright enough to test by sodium light. My assistant in the machine work scraped three 8 inch plates that were tested by this method and found to be very excellent, though it must be evident that a single cut of the scraper would change the spot over which it passed so much, as to entirely change the appearance there, but I found I could use the test to get the general outline of the surface under process of correction. These iron plates I would say are simply used for preliminary formation of polishers. I may have something to say on the question of surface plates in the future, as I have made some interesting studies on the subject. I must now bring this paper to a close, although I had intended including some interesting studies of curved surfaces. There is, however, matter enough in that subject of itself, especially when we connect it with the idiosyncrasies of the material we have to deal with, a vital part of the subject that I have not touched upon in the present paper. You may now inquire, how critical is this "color test." To answer this I fear I shall trench upon forbidden grounds, but I call to my help the words of one of our best American Physicists, and I quote from a letter in which he says by combined calculation and experiment I have found the limiting error for white light to be $\frac{1}{50000000}$ of an inch, and for Na or sodium light about fifty times greater or less than $\frac{1}{800000}$ of an inch. Dr. Alfred Mayer estimated and demonstrated by actual experiment that the smallest black spot on a white ground visible to the naked eye is about $\frac{1}{800}$ of an inch at the distance of normal vision, namely 10 inches, and that a line, which of course has the element of extension $\frac{1}{5000}$ of an inch in thickness could be seen. In our delicate "color test" we may decrease

the diameter of our black spot a thousand times and still its perception is possible by the aid of our monocromatic light, and we may diminish our line ten thousand times, yet find it just perceivable on the border land of our test by white light. Do not presume I am so foolish as to even think that the human hand directed by the human brain, can ever work the material at his command to such a high standard of exactness. No; from the very nature of the material we have to work with, we are forbidden even to hope for such an achievement, and could it be possible that through some stroke of good fortune, we could attain this high ideal, it would be but for a moment, as from the very nature of our environment, it would be but an Ignüs Fatuus. There is, however, to the earnest mind a delight in having a high model of excellence, for as our model is so will our work approximate, and although we may go on approximating *our* ideal forever we can never hope to reach that which has been set for us by the great Master Workman.